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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 287. 1900. Vol. XXIII.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of the disk of Mercury and that of Jupiter V. are from the American 'Nautical Almanac.'

We are indebted to Mr. Crommelin for the approximate Ephemeris of Eros near the time of its opposition, which is computed from Chandler's elements in Ast. Journ. No. 452, without taking account of perturbations.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

Date.	Day of Year.	Fr. of Year.	Sun		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 7	7	·016	4 7	20 6	22 24 S	0 6 24	19 6 23	● 1 1 52
14	14	·036	4 16	20 2	21 20	9 12	19 33 59	☽ 7 17 40
21	21	·055	4 28	19 54	19 56	11 28	20 1 35	○ 15 7 8
28	28	·074	4 40	19 46	18 14	13 8	20 29 11	☾ 23 11 53
								● 30 13 23
Feb. 4	35	·093	4 52	19 35	16 16	14 8	20 56 46	☽ 6 4 27
11	42	·112	5 4	19 23	14 4	14 27	21 24 22	○ 14 1 50
18	49	·131	5 18	19 9	11 41	14 9	21 51 58	☾ 22 4 44
25	56	·151	5 30	18 55	9 9	13 17	22 19 34	● 28 23 25
Mar. 4	63	·170	5 42	18 40	6 30	11 58	22 47 10	☽ 7 17 34
11	70	·169	5 55	18 24	3 46	10 15	23 14 46	○ 15 20 12
18	77	·208	6 7	18 8	1 1 S	8 17	23 42 22	☾ 23 17 36
25	84	·227	6 18	17 52	1 45 N	6 10	0 9 58	● 30 8 30
April 1	91	·246	6 31	17 36	4 29	4 2	0 37 33	☽ 6 8 55
8	98	·266	6 41	17 20	7 8	1 59	1 5 9	○ 14 13 2
15	105	·285	6 53	17 5	9 42	0 0 7	1 32 45	☾ 22 2 33
22	112	·304	7 5	16 50	12 8	23 58 31	2 0 21	● 28 17 23
29	119	·323	7 16	16 37	14 25	57 19	2 27 57	
May 6	126	·342	7 27	16 24	16 29	56 32	2 55 33	☽ 6 1 39
13	133	·361	7 39	16 12	18 20	56 11	3 23 9	○ 14 3 37
20	140	·381	7 50	16 2	19 57	56 19	3 50 45	☾ 21 8 31
27	147	·400	7 58	15 54	21 16	56 53	4 18 20	● 28 2 50
June 3	154	·419	8 7	15 49	22 18	57 52	4 45 56	☽ 4 18 59
10	161	·438	8 13	15 45	23 0	23 59 7	5 13 32	○ 12 15 38
17	168	·457	8 18	15 44	23 23	0 0 33	5 41 8	☾ 19 12 57
24	175	·476	8 19	15 45	23 26	2 4	6 8 44	● 26 13 27
July 1	182	·496	8 18	15 49	23 8	3 31	6 36 20	☽ 4 12 14
8	189	·515	8 15	15 55	22 31	4 46	7 3 56	○ 12 1 22
15	196	·534	8 10	16 3	21 34	5 40	7 31 32	☾ 18 17 31
2	203	·553	8 2	16 12	20 20	6 11	7 59 8	● 26 1 43
29	210	·572	7 52	16 22	18 49	6 15	8 26 44	
Aug. 5	217	·591	7 40	16 32	17 3	5 50	8 54 19	☽ 3 4 46
12	224	·611	7 28	16 43	15 3	4 54	9 21 55	○ 10 9 30
19	231	·630	7 14	16 54	12 51	3 32	9 49 31	☾ 16 23 46
26	238	·649	7 0	17 5	10 30	0 1 46	10 17 7	● 24 15 53
Sept. 2	245	·668	6 44	17 16	8 0	23 59 41	10 44 43	☽ 1 19 56
9	252	·687	6 28	17 28	5 24	57 21	11 12 19	○ 8 17 6
16	259	·706	6 12	17 39	2 43	54 54	11 39 55	☾ 15 8 57
23	266	·726	5 56	17 50	0 0 N	52 26	12 7 31	● 23 7 57
30	273	·745	5 40	18 1	2 43 S	50 5	12 35 6	
Oct. 7	280	·764	5 24	18 12	5 26	47 56	13 2 42	☽ 1 9 11
14	287	·783	5 8	18 24	8 5	46 6	13 30 18	○ 8 1 18
21	294	·802	4 55	18 36	10 38	44 44	13 57 54	☾ 14 21 51
28	301	·821	4 40	18 49	13 4	43 54	14 25 30	● 23 1 27
								☽ 30 20 17
Nov. 4	308	·841	4 27	19 2	15 20	43 40	14 53 6	○ 6 11 0
11	315	·860	4 16	19 14	17 23	44 6	15 20 42	☾ 13 14 37
18	322	·879	4 6	19 26	19 12	45 14	15 48 18	● 21 19 17
25	329	·898	3 58	19 38	20 44	47 3	16 15 53	☽ 29 5 35
Dec. 2	336	·917	3 52	19 48	21 57	49 28	16 43 29	○ 5 22 38
9	343	·936	3 49	19 57	22 49	52 23	17 11 5	☾ 13 10 42
16	350	·956	3 49	20 3	23 19	55 40	17 38 41	● 21 12 1
23	357	·975	3 52	20 7	23 27	23 59 9	18 6 17	☽ 28 13 48
30	364	·994	3 57	20 8	23 11 S	0 2 37	18 33 53	

***For Moon's phases:* ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.**

JANUARY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° '	°
1	M	4 11	......	20 3 S	+80·9 M.
2	Tu	5 33	0 58	16 26	+68·8 M.
3	W	6 57	1 55	11 47	+56·6 M.
4	Th	8 23	2 50	6 27	+44·5 M.
5	F	9 46	3 42	0 48 S	+32·3 M.
6	S	11 9	4 33	4 47 N	+20·1 M.
7	**S**	12 27	5 23	10 1	+ 8·0 M.
8	M	13 46	6 14	14 38	− 4·2 M.
9	Tu	15 3	7 6	18 24	−16·3 M.
10	W	16 14	7 58	21 8	−28·5 M.
11	Th	17 17	8 52	22 43	−40·6 M.
12	F	18 11	9 46	23 4	−52·8 M.
13	S	18 54	10 38	22 15	−64·9 M.
14	**S**	19 30	11 29	20 20	−77·1 M.
15	M	rises	12 17	17 31	−89·3 M.
16	Tu	5 33	13 3	13 59	+78·6 E.
17	W	6 41	13 46	9 55	+66·4 E.
18	Th	7 47	14 28	5 31	+54·2 E.
19	F	8 52	15 8	0 56 N	+42·1 E.
20	S	9 58	15 49	3 40 S	+30·0 E.
21	**S**	11 4	16 31	8 10	+17·8 E.
22	M	12 12	17 14	12 24	+ 5·7 E.
23	Tu	13 21	18 0	16 12	− 6·4 E.
24	W	14 31	18 50	19 23	−18·6 E.
25	Th	15 38	19 43	21 42	−30·8 E.
26	F	16 42	20 39	22 54	−43·0 E.
27	S	17 38	21 38	22 47	−55·1 E.
28	**S**	18 24	22 38	21 13	−67·3 E.
29	M	19 0	23 37	18 13	−79·5 E.
	Tu	sets	......	13 58	+88·3 M.

FEBRUARY.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° '	°
1	Th	7 20	1 30	3 4 S	+64·0 M.
2	F	8 45	2 23	2 46 N	+51·8 M.
3	S	10 10	3 16	8 19	+39·6 M.
4	**S**	11 31	4 8	13 17	+27·5 M.
5	M	12 50	5 1	17 23	+15·3 M.
6	Tu	14 4	5 55	20 25	+ 3·2 M.
7	W	15 10	6 49	22 18	− 8·9 M.
8	Th	16 6	7 42	22 57	−21·1 M.
9	F	16 54	8 35	22 25	−33·3 M.
10	S	17 30	9 25	20 49	−45·4 M.
11	**S**	18 0	10 14	18 16	−57·6 M.
12	M	18 25	11 0	14 58	−69·8 M.
13	Tu	18 45	11 44	11 6	−81·9 M.
14	W	rises	12 26	6 49	+85·9 E.
15	Th	6 42	13 7	2 18 N	+73·7 E.
16	F	7 48	13 48	2 17 S	+61·5 E.
17	S	8 54	14 29	6 48	+49·3 E.
18	**S**	10 0	15 12	11 5	+37·2 E.
19	M	11 7	15 56	14 57	+25·0 E.
20	Tu	12 15	16 43	18 16	+12·8 E.
21	W	13 23	17 33	20 49	+ 0·7 E.
22	Th	14 26	18 27	22 23	−11·5 E.
23	F	15 23	19 23	22 47	−23·7 E.
24	S	16 11	20 20	21 52	−35·9 E.
25	**S**	16 52	21 18	19 34	−48·0 E.
26	M	17 26	22 15	15 59	−60·2 E.
27	Tu	17 52	23 11	11 17	−72·4 E.
28	W	18 18	......	5 49 S	−84·5 E.

MARCH.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° '	°
1	Th	6 12	0 7	0 3 N	+83·3 M.
2	F	7 38	1 1	5 52	+71·1 M.
3	S	9 4	1 55	11 15	+58·9 M.
4	**S**	10 27	2 50	15 49	+46·8 M.
5	M	11 47	3 46	19 20	+34·6 M.
6	Tu	12 58	4 41	21 37	+22·4 M.
7	W	14 0	5 36	22 38	+10·3 M.
8	Th	14 50	6 30	22 25	− 1·9 M.
9	F	15 30	7 22	21 5	−14·1 M.
10	S	16 2	8 11	18 47	−26·3 M.
11	**S**	16 29	8 58	15 42	−38·5 M.
12	M	16 49	9 42	12 0	−50·7 M.
13	Tu	17 10	10 25	7 52	−62·9 M.
14	W	17 28	11 6	3 27 N	−75·0 M.
15	Th	17 44	11 47	1 5 S	−87·2 M.
16	F	rises	12 28	5 36	+80·5 E.
17	S	7 51	13 11	9 55	+68·4 E.
18	**S**	8 58	13 55	13 53	+56·3 E.
19	M	10 5	14 41	17 19	+44·1 E.
20	Tu	11 13	15 30	20 1	+31·9 E.
21	W	12 16	16 21	21 49	+19·8 E.
22	Th	13 14	17 15	22 31	+ 7·6 E.
23	F	14 5	18 10	22 1	− 4·6 E.
24	S	14 46	19 5	20 15	−16·8 E.
25	**S**	15 22	20 1	17 15	−28·9 E.
26	M	15 51	20 55	13 9	−41·1 E.
27	Tu	16 16	21 50	8 10	−53·3 E.
28	W	16 40	22 43	2 35 S	−65·5 E.
29	Th	17 4	23 38	3 11 N	−77·7 E.
30	F	17 28	......	8 47	−89·9 E.

APRIL.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° '	°
1	**S**	9 19	1 29	17 49 N	+65·7 M.
2	M	10 36	2 27	20 39	+53·5 M.
3	Tu	11 45	3 24	22 10	+41·3 M.
4	W	12 43	4 21	22 21	+29·2 M.
5	Th	13 27	5 15	21 20	+17·0 M.
6	F	14 3	6 6	19 16	+ 4·7 M.
7	S	14 31	6 54	16 22	− 7·5 M.
8	**S**	14 55	7 40	12 50	−19·7 M.
9	M	15 15	8 23	8 50	−31·9 M.
10	Tu	15 34	9 4	4 31	−44·0 M.
11	W	15 50	9 46	0 3 N	−56·2 M.
12	Th	16 7	10 27	4 27 S	−68·4 M.
13	F	16 28	11 9	8 49	−80·6 M.
14	S	rises	11 52	12 52	+87·2 E.
15	**S**	7 55	12 38	16 26	+75·0 E.
16	M	9 3	13 27	19 20	+62·8 E.
17	Tu	10 8	14 18	21 20	+50·6 E.
18	W	11 9	15 11	22 17	+38·5 E.
19	Th	12 1	16 5	22 3	+26·3 E.
20	F	12 46	17 0	20 36	+14·1 E.
21	S	13 22	17 54	17 58	+ 1·8 E.
22	**S**	13 51	18 47	14 16	−10·5 E.
23	M	14 17	19 39	9 42	−22·7 E.
24	Tu	14 41	20 31	4 30 S	−34·9 E.
25	W	15 4	21 24	1 2 N	−47·0 E.
26	Th	15 27	22 17	6 33	−59·2 E.
27	F	15 54	23 12	11 43	−71·4 E.
28	S	sets	......	16 8	−83·7 E.
29	**S**	8 8	0 9	19 30	+84·1 M.
30	M	9 23	1 7	21 35 N	+71·9 M.

MAY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	10 28	2 5	22 17 N	+59·7 M.
2	W	11 19	3 2	21 44	+47·5 M.
3	Th	11 59	3 56	19 56	+35·3 M.
4	F	12 31	4 47	17 14	+23·2 M.
5	S	12 57	5 34	13 50	+11·0 M.
6	**S**	13 18	6 19	9 56	− 1·3 M.
7	M	13 37	7 1	5 42	−13·5 M.
8	Tu	13 56	7 42	1 16 N	−25·7 M.
9	W	14 12	8 23	3 13 S	−37·9 M.
10	Th	14 30	9 5	7 26	−50·1 M.
11	F	14 52	9 48	11 45	−62·4 M.
12	S	15 16	10 34	15 29	−74·6 M.
13	**S**	15 46	11 21	18 35	−86·8 M.
14	M	rises	12 12	20 51	+81·0 E.
15	Tu	9 2	13 6	22 5	+68·8 E.
16	W	9 59	14 0	22 8	+56·6 E.
17	Th	10 45	14 56	20 57	+44·4 E.
18	F	11 24	15 50	18 33	+32·2 E.
19	S	11 56	16 44	15 5	+20·0 E.
20	**S**	12 22	17 35	10 44	+ 7·7 E.
21	M	12 46	18 26	5 46	− 4·5 E.
22	Tu	13 7	19 17	0 27 S	−16·7 E.
23	W	13 30	20 8	4 56 N	−28·9 E.
24	Th	13 55	21 1	10 5	−41·0 E.
25	F	14 21	21 55	14 39	−53·3 E.
26	S	14 54	22 52	18 21	−65·5 E.
27	**S**	15 36	23 50	20 54	−77·8 E.
28	M	sets	……	22 9	−90·0 E.
29	Tu	9 7	0 47	22 2	+77·8 M.
30	W	9 54	1 43	20 41	+65·6 M.

JUNE.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	10 59	3 26	15 4 N	+41·1 M.
2	S	11 21	4 12	11 16	+28·9 M.
3	**S**	11 42	4 56	7 5	+16·7 M.
4	M	12 0	5 38	2 41 N	+ 4·5 M.
5	Tu	12 17	6 19	1 48 S	− 7·8 M.
6	W	12 35	7 0	6 13	−20·0 M.
7	Th	12 55	7 43	10 27	−32·2 M.
8	F	13 17	8 27	14 18	−44·4 M.
9	S	13 45	9 14	17 38	−56·6 M.
10	**S**	14 17	10 3	20 12	−68·8 M.
11	M	15 0	10 56	21 49	−81·1 M.
12	Tu	rises	11 51	22 15	+86·7 E.
13	W	8 41	12 48	21 26	+74·5 E.
14	Th	9 23	13 44	19 19	+62·3 E.
15	F	9 58	14 39	16 3	+50·1 E.
16	S	10 26	15 32	11 51	+37·9 E.
17	**S**	10 52	16 24	6 57	+25·6 E.
18	M	11 12	17 14	1 40 S	+13·3 E.
19	Tu	11 35	18 5	3 41 N	+ 1·1 E.
20	W	11 58	18 56	8 50	−11·1 E.
21	Th	12 25	19 49	13 30	−23·3 E.
22	F	12 53	20 43	17 23	−35·6 E.
23	S	13 32	21 39	20 14	−47·8 E.
24	**S**	14 17	22 36	21 53	−60·0 E.
25	M	15 13	23 32	22 14	−72·2 E.
26	Tu	sets	……	21 19	−84·5 E.
27	W	8 27	0 26	19 17	+83·3 M.
28	Th	8 59	1 17	16 20	+71·1 M.
29	F	9 25	2 5	12 42	+58·9 M.
30	S	9 46	2 50	8 36 N	+46·7 M.

JULY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	10 5	3 33	4 14 N	+34·5 M.
2	M	10 24	4 15	0 15 S	+22·2 M.
3	Tu	10 40	4 56	4 42	+10·0 M.
4	W	11 0	5 38	8 58	− 2·2 M.
5	Th	11 19	6 20	12 57	−14·4 M.
6	F	11 44	7 5	16 27	−26·7 M.
7	S	12 15	7 53	19 18	−38·9 M.
8	**S**	12 52	8 44	21 17	−51·1 M.
9	M	13 41	9 38	22 12	−63·3 M.
10	Tu	14 42	10 34	21 53	−75·6 M.
11	W	15 51	11 32	20 14	−87·8 M.
12	Th	rises	12 28	17 20	+80·0 E.
13	F	8 29	13 24	13 20	+67·8 E.
14	S	8 55	14 18	8 30	+55·6 E.
15	**S**	9 20	15 10	3 11 S	+43·4 E.
16	M	9 42	16 2	2 17 N	+31·2 E.
17	Tu	10 5	16 53	7 34	+19·0 E.
18	W	10 30	17 46	12 23	+ 6·7 E.
19	Th	10 58	18 39	16 27	− 5·5 E.
20	F	11 32	19 34	19 33	−17·7 E.
21	S	12 14	20 29	21 30	−29·9 E.
22	**S**	13 5	21 25	22 12	−42·1 E.
23	M	14 4	22 19	21 40	−54·3 E.
24	Tu	15 9	23 10	16 0	−66·5 E.
25	W	16 17	23 59	17 22	−78·7 E.
26	Th	sets	……	13 58	+89·1 M.
27	F	7 50	0 45	10 2	+76·7 M.
28	S	8 11	1 29	5 45	+64·5 M.
29	**S**	8 30	2 11	1 18 N	+52·3 M.
30	M	8 47	2 53	3 10 S	+40·1 M.

AUGUST.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	9 24	4 16	11 32 S	+15·6 M.
2	Th	9 47	4 59	15 10	+ 3·4 M.
3	F	10 15	5 4[illegible]	18 13	− 8·8 M.
4	S	10 4[illegible]	6 34	20 30	−21·1 M.
5	**S**	11 30	7 25	21 50	−33·3 M.
6	M	12 24	8 20	22 2	−45·5 M.
7	Tu	13 29	9 16	20 59	−57·7 M.
8	W	14 43	10 13	18 38	−69·9 M.
9	Th	16 3	11 9	15 5	−82·1 M.
10	F	rises	12 5	10 31	+85·6 E.
11	S	7 22	12 59	5 16 S	+73·4 E.
12	**S**	7 46	13 53	0 19 N	+61·2 E.
13	M	8 9	14 46	5 50	+49·0 E.
14	Tu	8 35	15 40	10 56	+36·8 E.
15	W	9 3	16 35	15 19	+24·6 E.
16	Th	9 35	17 30	18 43	+12·4 E.
17	F	10 16	18 26	20 57	+ 0·1 E.
18	S	11 3	19 21	21 57	−12·1 E.
19	**S**	11 59	20 15	21 44	−24·3 E.
20	M	13 1	21 6	20 22	−36·5 E.
21	Tu	14 7	21 56	18 1	−48·7 E.
22	W	15 11	22 4[illegible]	14 53	−60·9 E.
23	Th	16 24	23 26	11 9	−73·2 E.
24	F	sets	……	7 0	−85·4 E.
25	S	6 36	0 9	2 38 N	+82·4 M.
26	**S**	6 56	0 51	1 48 S	+70·2 M.
27	M	7 11	1 32	6 8	+58·0 M.
28	Tu	7 38	2 14	10 14	+45·8 M.
29	W	7 53	2 56	13 57	+33·6 M.
30	Th	8 18	3 41	17 8	+21·4 M.

SEPTEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	9 27	5 17	20 15 S	− 3·1 M.
2	**S**	10 13	6 8	21 52	−15·3 M.
3	M	11 12	7 2	21 20	−27·5 M.
4	Tu	12 18	7 57	19 34	−39·7 M.
5	W	13 34	8 53	16 37	−51·9 M.
6	Th	14 55	9 48	12 33	−64·1 M.
7	F	16 20	10 43	7 38	−76·3 M.
8	S	rises	11 38	2 10 S	−88·5 M.
9	**S**	6 10	12 32	3 29 N	+79·3 E.
10	M	6 36	13 28	8 54	+67·1 E.
11	Tu	7 3	14 24	13 41	+55·0 E.
12	W	7 36	15 21	17 32	+42·8 E.
13	Th	8 15	16 18	20 11	+30·6 E.
14	F	9 0	17 15	21 34	+18·4 E.
15	S	9 55	18 11	21 39	+ 6·2 E.
16	**S**	10 56	19 3	20 33	− 6·1 E.
17	M	12 0	19 53	18 27	−18·3 E.
18	Tu	13 8	20 40	15 31	−30·5 E.
19	W	14 15	21 25	11 58	−42·7 E.
20	Th	15 21	22 8	7 58	−54·9 E.
21	F	16 27	22 50	3 43 N	−67·1 E.
22	S	17 32	23 31	0 40 S	−79·3 E.
23	**S**	sets	……	4 59	+88·5 M.
24	M	5 39	0 12	9 8	+76·3 M.
25	Tu	6 0	0 55	12 55	+64·1 M.
26	W	6 24	1 39	16 13	+51·8 M.
27	Th	6 53	2 25	18 51	+39·6 M.
28	F	7 27	3 13	20 41	+27·4 M.
29	S	8 11	4 3	21 33	+15·2 M.

OCTOBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	10 3	5 47	20 3 S	− 9·1 M.
2	Tu	11 15	6 41	17 37	−21·3 M.
3	W	12 30	7 34	14 6	−33·5 M.
4	Th	13 50	8 27	9 41	−45·7 M.
5	F	15 12	9 21	4 34	−57·9 M.
6	S	16 38	10 15	0 55 S	−70·0 M.
7	**S**	18 3	11 10	6 26 N	−82·2 M.
8	M	rises	12 6	11 33	+85·6 E.
9	Tu	5 30	13 4	15 54	+73·4 E.
10	W	6 7	14 4	19 7	+61·2 E.
11	Th	6 52	15 3	21 1	+49·0 E.
12	F	7 46	16 1	21 32	+36·8 E.
13	S	8 46	16 57	20 46	+24·6 E.
14	**S**	9 52	17 49	18 54	+12·4 E.
15	M	10 58	18 37	16 9	+ 0·3 E.
16	Tu	12 6	19 23	12 44	−11·9 E.
17	W	13 12	20 7	8 51	−24·1 E.
18	Th	14 18	20 49	4 40	−36·2 E.
19	F	15 23	21 30	0 21 N	−48·4 E.
20	S	16 28	22 11	3 57 S	−60·5 E.
21	**S**	17 33	22 53	8 7	−72·7 E.
22	M	18 38	23 37	12 0	−84·9 E.
23	Tu	sets	……	15 24	+82·9 M.
24	W	4 57	0 23	18 12	+70·7 M.
25	Th	5 29	1 10	20 13	+58·5 M
26	F	6 10	2 0	21 18	+46·3 M.
27	S	6 59	2 51	21 21	+34·1 M
28	**S**	7 57	3 43	20 19	+21·9 M.
29	M	9 2	4 35	18 12	+ 9·8 M.

NOVEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	12 48	7 10	6 23 S	−26·7 M.
2	F	14 8	8 1	1 12 S	−38·9 M.
3	S	15 32	8 54	4 9 N	−51·1 M.
4	**S**	16 54	9 48	9 21	−63·3 M.
5	M	18 20	10 45	14 0	−75·5 M.
6	Tu	rises	11 44	17 45	−87·7 M.
7	W	4 40	12 44	20 15	+80·3 E.
8	Th	5 30	13 44	21 23	+68·1 E.
9	F	6 29	14 43	21 6	+55·9 E.
10	S	7 33	15 38	19 35	+43·7 E.
11	**S**	8 43	16 30	17 2	+31·5 E.
12	M	9 53	17 18	13 45	+19·3 E.
13	Tu	11 2	18 3	9 50	+ 7·1 E.
14	W	12 7	18 46	5 47	− 5·0 E.
15	Th	13 13	19 28	1 29 N	−17·2 E.
16	F	14 18	20 9	2 50 S	−29·3 E.
17	S	15 22	20 51	7 2	−41·5 E.
18	**S**	16 28	21 34	10 59	−53·7 E.
19	M	17 32	22 19	14 32	−65·9 E.
20	Tu	18 37	23 6	17 31	−78·0 E.
21	W	19 39	23 55	19 45	+89·9 M.
22	Th	sets	……	21 6	+77·7 M.
23	F	4 55	0 47	21 25	+65·5 M.
24	S	5 50	1 39	20 37	+53·3 M.
25	**S**	6 55	2 32	18 44	+41·1 M.
26	M	8 5	3 24	15 50	+29·0 M.
27	Tu	9 19	4 15	12 3	+16·8 M.
28	W	10 36	5 6	7 36	+ 4·7 M.
29	Th	11 53	5 55	2 40 S	− 7·4 M.

DECEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	14 31	7 37	7 35 N	−31·7 M.
2	**S**	15 52	8 30	12 18	−43·9 M.
3	M	17 13	9 26	16 20	−56·1 M.
4	Tu	18 31	10 25	19 20	−68·3 M.
5	W	19 40	11 25	21 3	−80·5 M.
6	Th	rises	12 24	21 24	+87·4 E.
7	F	5 11	13 22	20 23	+75·2 E.
8	S	6 22	14 17	18 12	+63·1 E.
9	**S**	7 32	15 8	15 7	+50·9 E.
10	M	8 44	15 56	11 23	+38·8 E.
11	Tu	9 51	16 40	7 16	+26·6 E.
12	W	10 58	17 23	2 57 N	+14·5 E.
13	Th	12 4	18 5	1 25 S	+ 2·3 E.
14	F	13 8	18 46	5 41	− 9·8 E.
15	S	14 13	19 29	9 44	−22·0 E.
16	**S**	15 19	20 13	13 25	−34·2 E.
17	M	16 24	20 59	16 36	−46·3 E.
18	Tu	17 28	21 48	19 6	−58·5 E.
19	W	18 28	22 39	20 46	−70·7 E.
20	Th	19 24	23 31	21 26	−82·9 E.
21	F	sets	……	21 0	+85·1 M.
22	S	4 43	0 25	19 24	+72·9 M.
23	**S**	5 54	1 19	16 43	+60·7 M.
24	M	7 8	2 11	13 6	+48·5 M.
25	Tu	8 24	3 3	8 44	+36·3 M.
26	W	9 40	3 53	3 53 S	+24·1 M.
27	Th	10 59	4 43	1 12 N	+12·0 M.
28	F	12 18	5 34	6 16	0·0 M.
29	S	13 36	6 25	11 0	−12·2 M.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α	δ			α	δ	
Jan. 2 ...	**230°**	**+53°**	Swift; long paths.	July 28 ...	**339°**	**−12°**	Slow; long.
3 ...	156	+41	Swift.	30 ...	6	+35	Swift; streaks.
11 ...	220	+13	Swift; streaks.	Aug. 4 ...	30	+36	Swift; streaks.
17 ...	295	+53	Slow; bright.	4-10	350	+51	Swift.
22 ...	208	− 8	Very swift; streaks	10 ...	**45**	**+57**	Swift; streaks.
25 ..	131	+32	Swift.	14 ...	311	+62	Rather swift.
29 ...	213	+52	Very swift.	15 ...	292	+53	Swift; bright.
Feb. 5-10	74	+43	Slow; bright.	16 ...	61	+48	Swift; streaks.
15 ...	236	+11	Swift; streaks.	22 ...	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	5	+11	Slow; short.
20 ...	181	+34	Swift; bright.	Sept. 4-20	346	± 0	Slow.
20 ...	263	+36	Swift; streaks.	5-15	62	+37	Swift; streaks.
Mar. 1-4...	166	+ 4	Slow; bright.	6-17	106	+52	Swift; streaks.
14 ...	250	+54	Swift.	10 ...	74	+41	Swift; streaks.
18 ..	316	+76	Slow; bright.	15 ...	77	+57	Swift; streaks.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	335	+58	Slowish.
28 ...	263	+62	Rather swift.	27 ...	75	+15	Swift; streaks.
Apr. 12-24	210	−10	Slow fireballs.	Oct. 2 ...	230	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
18-22	296	± 0	Swift; streaks.	4 ...	310	+77	Slowish.
19 ...	229	− 2	Slow; long.	8 ...	77	+31	Swift; streaks.
20 ...	**270**	**+33**	wift.	8-14	45	+58	Small; short.
25 ...	272	+21	Swift; short.	14 ...	133	+68	Rather swift.
30 ...	291	+59	Rather slow.	18 ...	**92**	**+15**	Swift; streaks.
May 1 ...	239	+46	Small; short.	20 ...	106	+12	Swift; streaks.
5 ...	254	−21	Slowish.	29 ...	109	+23	Very swift.
6 ...	**338**	**− 2**	Swift; streaks.	Nov. 1 ...	43	+22	Slow; bright.
7 ...	244	+ 7	Slow; bright.	2 ...	55	+ 9	Slow; bright.
11 ...	231	+27	Slow; small.	7 ...	102	+73	Very swift.
29 ...	264	+64	Slowish.	10-12	133	+31	Very swift; streaks
30 ...	333	+27	Swift; streaks.	14-15	**150**	**+22**	Swift; streaks.
June 2-17..	249	−20	Slow; fireballs.	16-28	154	+41	Swift; streaks.
10 ...	261	+ 5	Very slow.	20-23	62	+23	Slow; bright.
11-19	274	+69	Rather swift.	23-24	**25**	**+44**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-28	208	+43	Very swift.
15 ...	291	+52	Swift; small.	30 ...	190	+58	Swift; streaks.
20 ...	335	+57	Swift.	Dec. 4 ...	162	+58	Swift; streaks.
26 ...	354	+39	Swift; streaks.	6 ...	80	+23	Slow; bright.
July 4 ...	303	+24	Swift.	8 ...	145	+ 7	Swift; streaks.
7 ...	333	+26	Swift.	8 ...	208	+71	Rather swift.
6-12	282	−13	Very slow.	10-12	**108**	**+33**	Swift; short.
19 ...	314	+48	Swift; short.	12 ...	119	+29	Rather swift.
22 ...	16	+31	Swift; streaks.	22 ...	194	+67	Swift; streaks.
25-Sep. 15	48	+43	Swift; streaks.	25 ...	98	+31	Very slow.

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November will probably furnish brilliant displays in 1900 and 1901.

The Perseids, with max. on August 10, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α	δ		α	δ		α	δ
July 19 ...	19·7°	+50·9°	July 29 ...	30·8°	+53·8°	Aug. 8 ...	42·6°	+56·5°
21 ...	21·8	+51·6	31 ...	33·2	+54·4	10 ...	45·0	+57·0
23 ...	24·0	+52·2	Aug. 2 ...	35·5	+55·0	12 ...	47·5	+57·5
25 ...	26·2	+52·8	4 ...	37·9	+55·5	14 ...	50·1	+58·0
27 ...	28·5	+53·3	6 ...	40·2	+56·0	16 ...	52·6	+58·5

1900.	Mercury (☿).						Venus (♀).						Mars (♂).					
	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	17 15 16	22 0 S.	5·8	18 31	22 34	2 37	20 39 24	20 10 S.	11·4	21 42	1 57	6 12	19 2 20	23 37 S.	4·0	20 29	0 20	4 11
11...	18 15 9	23 41	5·2	19 4	22 55	2 46	21 29 31	16 37	11·8	21 31	2 7	6 43	19 35 43	22 37	4·0	20 16	0 14	4 12
21...	19 21 2	23 30	4·8	19 29	23 22	3 15	22 17 23	12 19	12·2	21 16	2 16	7 16	20 8 44	21 12	4·0	19 59	0 7	4 15
31...	20 29 43	21 2	4·8	19 42	23 51	4 0	23 3 16	7 29	12·8	20 56	2 22	7 48	20 41 14	19 24	4·0	19 40	0 0 23 59	4 20
Feb. 10...	21 39 30	16 9	4·8	19 40	0 19	4 58	23 47 43	2 20 S.	13·4	20 35	2 27	8 19	21 13 5	17 14	4·0	19 20	23 52	4 24
20...	22 48 51	8 53	5·2	19 30	0 49	6 8	0 31 18	2 55 N.	14·0	20 13	2 31	8 49	21 44 14	14 46	4·0	18 57	23 43	4 29
Mar. 2...	23 51 26	0 19 S.	6·2	19 9	1 12	7 15	1 14 39	8 4	14·8	19 50	2 35	9 20	22 14 41	12 3	4·0	18 33	23 34	4 35
12...	0 26 52	5 54 N.	8·2	18 35	1 8	7 41	1 58 19	12 56	15·6	19 29	2 40	9 51	22 44 29	9 9	4·0	18 8	23 25	4 42
22...	0 17 20	5 37	10·8	17 49	0 19	6 49	2 42 39	17 18	16·8	19 8	2 45	10 22	23 13 44	6 7	4·0	17 41	23 15	4 47
Apr. 1...	23 51 28	0 43 N.	11·0	17 1	23 8	5 15	3 27 46	21 1	18·0	18 50	2 50	10 50	23 42 32	2 59 S.	4·0	17 15	23 4	4 53
11...	23 51 0	2 2 S.	9·6	16 38	22 32	4 24	4 13 25	23 55	19·8	18 35	2 56	11 17	0 11 2	0 9 N.	4·0	16 48	22 53	4 58
21...	0 17 33	0 48 S.	8·0	16 21	22 21	4 21	4 58 46	25 52	21·8	18 27	3 2	11 37	0 39 21	3 16	4·2	16 23	22 42	5 1
May 1...	1 1 17	3 25 N.	6·8	16 11	22 26	4 41	5 42 37	26 51	24·2	18 24	3 7	11 50	1 7 37	6 17	4·2	15 57	22 31	5 4
11...	1 58 6	9 36	5·8	15 54	22 45	5 36	6 23 13	26 53	27·4	18 25	3 8	11 51	1 35 56	9 12	4·2	15 30	22 20	5 10
21...	3 9 36	16 44	5·2	15 44	23 19	6 54	6 58 22	26 6	31·4	18 27	3 4	11 41	2 4 24	11 55	4·2	15 4	22 9	5 14
31...	4 37 27	22 54	5·0	15 48	0 3	8 18	7 25 32	24 43	36·4	18 24	2 51	11 17	2 33 6	14 27	4·2	14 39	21 58	5 17
June 10...	6 9 48	25 20	5·4	16 24	0 56	9 28	7 41 26	22 59	42·8	18 15	2 28	10 41	3 2 3	16 41	4·2	14 14	21 47	5 20
20...	7 28 2	23 46	6·2	17 15	1 35	9 55	7 42 21	21 10	50·0	17 49	1 49	9 49	3 31 15	18 42	4·4	13 52	21 37	5 22
30...	8 25 14	19 57	7·4	17 59	1 53	9 47	7 26 52	19 27	56·2	17 6	0 54	8 42	4 0 41	20 23	4·4	13 31	21 27	5 23
July 10...	8 59 19	15 43	9·0	18 21	1 47	9 13	7 0 56	18 1	57·8	16 5	23 43	7 21	4 30 15	21 44	4·4	13 13	21 18	5 23
20...	9 5 50	12 49	10·6	18 5	1 14	8 23	6 38 46	17 8	53·8	15 8	22 42	6 16	4 59 49	22 44	4·4	12 56	21 8	5 20
30...	8 44 19	12 59	11·4	17 4	0 13	7 17	6 31 12	16 58	47·2	14 23	21 57	5 31	5 29 16	23 24	4·6	12 41	20 58	5 15
Aug. 9...	8 22 0	15 44	9·8	15 42	23 7	6 32	6 39 19	17 17	40·4	13 52	21 27	5 2	5 58 24	23 43	4·6	12 28	20 47	5 6
19...	8 38 46	17 35	7·4	15 11	22 49	6 27	6 59 55	17 40	34·6	13 29	21 9	4 49	6 27 5	23 43	4·8	12 18	20 37	4 56
29...	9 37 37	15 29	5·8	15 46	23 12	6 38	7 29 21	17 45	30·0	13 22	21 0	4 38	6 55 7	23 24	5·0	12 8	20 25	4 42
Sept. 8...	10 50 56	9 16	5·0	16 54	23 46	6 36	8 4 41	17 16	26·4	13 20	20 56	4 32	7 22 21	22 49	5·0	12 1	20 13	4 25
18...	11 58 54	1 28 N.	4·8	18 0	0 11	6 22	8 43 51	16 3	23·6	13 28	20 56	4 24	7 48 41	21 58	5·2	11 54	20 0	4 6
28...	12 59 55	6 11 S.	4·8	19 0	0 32	6 4	9 25 16	14 4	21·4	13 41	20 58	4 15	8 13 58	20 56	5·4	11 46	19 45	3 44
Oct. 8...	13 56 50	13 0	5·0	19 54	0 50	5 46	10 7 51	11 20	19·4	13 59	21 1	4 3	8 38 6	19 43	5·6	11 38	19 30	3 22
18...	14 51 12	18 35	5·6	20 40	1 5	5 30	10 50 58	7 57	18·0	14 20	21 5	3 50	9 1 2	18 24	6·0	11 31	19 14	2 57
28...	15 40 58	22 29	6·4	21 15	1 15	5 15	11 34 24	4 2 N.	16·6	14 45	21 9	3 33	9 22 37	17 0	6·2	11 21	18 56	2 31
Nov. 7...	16 13 51	23 52	7·8	21 20	1 9	4 56	12 18 9	0 13 S.	15·6	15 11	21 14	3 19	9 42 46	15 36	6·6	11 11	18 36	2 0
17...	15 58 1	20 56	9·8	20 5	0 13	4 21	13 2 32	4 38	14·8	15 38	21 19	3 0	10 1 21	14 13	7·0	10 58	18 15	1 32
27...	15 15 26	15 40	8·8	18 6	22 46	3 26	13 47 56	9 1	14·0	16 7	21 25	2 43	10 18 7	12 56	7·6	10 43	17 53	1 3
Dec. 7...	15 30 1	16 33	6·8	17 49	22 27	3 3	14 34 46	13 9	13·2	16 37	21 32	2 27	10 32 48	11 49	8·2	10 24	17 28	0 32
17...	16 19 9	20 11	5·6	18 23	22 38	2 53	15 23 25	16 48	12·8	17 7	21 42	2 17	10 45 1	10 55	8·8	10 1	17 0	23 59
27...	17 20 20	23 11 S.	5·0	19 5	23 0	2 55	16 4 3	19 45 S.	12·2	17 35	21 53	2 11	10 54 14	10 19 N.	9·5	9 34	16 30	23 26

Mercury.

d h d h d h d h
Gr. elong. E. Mar. 7 23; July 4 1; Oct. 29 16
„ W. Apr. 21 15; Aug. 19 2; Dec. 7 15
Sup. conj. ...Feb. 9 9; May 29 19; Sept. 13 5
Inf. conj. ...Mar. 24 15; July 31 20; Nov. 20 0
Stationary...Mar. 14 16; Apr. 6 2; July 17 4; Aug. 10 14

Venus.

d h d h
Gr. elong. E.... Apr. 28 12
„ W....Sept. 17 6
Inf. conj. ......July 7 23
Stationary......June 15 14; July 29 14
Gr. brilliancy..May 31 18; Aug. 13 20

Superior Planets.

	Conjunction.		Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h	d h
Mars	...Jan. 15 17;	Dec. 13 21		Nov. 21 19			
Jupiter	.. -	......	May 27 7	Feb. 28 6;	Aug. 25 9	Mar. 27 11;	July 28 20
Saturn	...Dec. 29 1		June 23 5	Mar. 25 8;	Sept. 21 17	Apr. 13 19;	Sept. 2 4
Uranus	...Dec. 4 19		May 31 23	Mar. 2 22;	Aug. 31 23	Mar. 17 8;	Aug. 17 8

Jupiter (♃). Saturn (♄).

1900.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.
	h m s	° ′	″	h m	h m	m h	h m s	° ′	″	h m	h m
Jan. 1	15 57 7	19 37 S.	29·8	16 54	21 12	1 30	17 50 23	22 26 S.	14·0	19 5	23 4
21	16 12 18	20 18	31·0	15 55	20 8	0 21	18 0 5	22 27	14·2	17 56	21 55
Feb. 10	16 24 46	20 48	32·8	14 53	19 2	23 11	18 8 39	22 27	14·4	16 46	20 45
Mar. 2	16 33 28	21 5	34·8	13 43	17 51	21 59	18 15 27	22 24	14·8	15 33	19 33
22	16 37 29	21 12	37·0	12 30	16 37	20 44	18 19 57	22 22	15·2	14 19	18 19
Apr. 11	16 36 17	21 8	39·2	11 9	15 17	19 25	18 21 46	22 20	15·8	13 2	17 2
May 1	16 30 8	20 55	41·0	9 42	13 52	18 2	18 20 46	22 20	16·2	11 42	15 42
21	16 20 29	20 34	42·0	8 13	12 24	16 35	18 17 11	22 22	16·6	10 20	14 20
June 10	16 9 55	20 10	41·8	6 40	10 54	15 8	18 11 43	22 25	17·0	8 57	12 56
30	16 1 15	19 50	40·6	5 11	9 27	13 43	18 5 25	22 28	17·0	7 32	11 31
July 20	15 56 36	19 41	38·8	3 47	8 4	12 21	17 59 32	22 30	16·8	6 7	10 6
Aug. 9	15 56 53	19 47	36·6	2 29	6 46	11 3	17 55 13	22 33	16·4	4 45	8 44
29	16 2 6	20 6	34·6	1 17	5 32	9 47	17 53 17	22 35	16·0	3 24	7 23
Sept. 18	16 11 42	20 36	32·6	0 12	4 23	8 34	17 54 6	22 39	15·4	2 8	6 5
Oct. 8	16 24 55	21 11	31·2	23 11	3 18	7 25	17 57 40	22 42	15·0	0 53	4 50
28	16 40 57	21 47	30·0	22 11	2 15	6 19	18 3 41	22 45	14·6	23 41	3 38
Nov. 17	16 59 0	22 20	29·4	21 14	1 14	5 14	18 11 39	22 46	14·2	22 30	2 27
Dec. 7	17 18 18	22 46	29·0	20 18	0 15	4 12	18 21 1	22 44	14·0	21 21	1 18
27	17 38 1	23 2 S.	29·2	19 18	23 13	3 8	18 31 6	22 39 S.	14·0	20 11	0 9

Uranus (♅). Neptune (♆).

1900.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.
Jan. 1	16 34 12	21 55 S.	3·4	17 45	21 48	1 51	5 39 19	22 4 N.	2·7	2 49	10 55
31	16 40 16	22 7	3·5	15 54	19 56	23 58	5 36 20	22 3	2·7	0 48	8 54
Mar. 2	16 43 39	22 14	3·6	14 0	18 1	22 2	5 35 2	22 4	2·6	22 49	6 55
Apr. 1	16 43 40	22 14	3·6	12 3	16 4	20 5	5 35 53	22 6	2·6	20 51	4 57
May 1	16 40 39	22 9	3·7	10 1	14 2	18 3	5 38 44	22 9	2·6	18 55	3 2
31	16 35 44	22 0	3·7	7 58	12 0	16 2	5 42 58	22 12	2·5	17 2	1 9
June 30	16 30 43	21 50	3·7	5 54	9 57	14 0	5 47 45	22 14	2·5	15 5	23 12
July 30	16 27 28	21 43	3·7	3 51	7 55	11 59	5 52 11	22 14	2·5	13 11	21 18
Aug. 29	16 27 7	21 42	3·6	1 53	5 57	10 1	5 55 25	22 14	2·6	11 16	19 23
Sept. 28	16 30 0	21 49	3·5	23 59	4 2	8 5	5 56 47	22 13	2·6	9 20	17 27
Oct. 28	16 35 37	22 1	3·4	22 8	2 10	6 12	5 56 2	22 12	2·6	7 21	15 28
Nov. 27	16 43 0	22 16	3·3	20 18	0 19	4 20	5 53 24	22 11	2·7	5 20	13 27
Dec. 27	16 50 47	22 29 S.	3·4	18 26	22 25	2 24	5 49 50	22 11 N.	2·7	3 19	11 26

Ceres. Pallas.

1900.	R.A. Noon.	Dec. Noon.	Transits.	1900.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m		h m	° ′	h m
July 6 ...	21 17	26 56 S.	14 18	June 12 ...	19 15	21 54 N.	13 51
18 ...	21 9	28 18	13 23	24 ...	19 6	22 14	12 55
30 ...	20 59	29 34	12 25	July 6 ...	18 56	21 55	11 58
Aug. 11 ...	20 48	30 34	11 27	18 ...	18 47	20 56	11 1
23 ...	20 38	31 11 S.	10 30	30 ...	18 38	19 22 N.	10 6

Juno. Vesta.

1900.	R.A. Noon.	Dec. Noon.	Transits.	1900.	R.A. Noon.	Dec. Noon.	Transits.
Sept. 4 ...	0 38	1 24 N.	13 43	Sept. 16 ...	8 1	20 5 N.	20 19
16 ...	0 33	1 0 S.	12 51	28 ...	8 20	19 26	19 50
28 ...	0 26	3 42	11 56	Oct. 10 ...	8 37	18 46	19 20
Oct. 10 ...	0 18	6 18	11 2	22 ...	8 52	18 8	18 48
22 ...	0 12	8 25 S.	10 8	Nov. 3 ...	9 6	17 36 N.	18 15

The times of **rising and setting correspond to the adjacent transit, irrespective of the day give** *the first column.*

ECLIPSES IN 1900.

In the year 1900 there will be two Eclipses of the Sun and one of the Moon.

I.—*A Total Eclipse of the Sun*, 1900, *May* 28.

Visible as a Partial Eclipse at Greenwich. The centre of the shadow after passing through Mexico, Georgia, South Carolina, and North Carolina, crosses the Atlantic and reaches the Continent of Europe on the coast of Portugal, not far from Oporto, where the duration of totality is about 94 seconds. Crossing Spain it leaves that country at a point near Alicante, and passes thence along the north coast of Africa ending its course in Egypt, where the middle of the Eclipse happens at Sunset.

	Eclipse begins.	Totality begins.	Totality ends.	Sun's Altitude.
	h m s	h m s	h m s	°
Ovar	2 43 7	4 1 42	4 3 15	42
Talavera (near) ...	2 49 58	4 6 42	4 8 9	39
Alicante	2 58 47	4 12 52	4 14 11	34
Algiers	3 5 9	4 17 20	4 18 22	30

G.M.T. in all cases.

Central Eclipse begins May 28^{d} 1^{h} $14^{m}\cdot 7$ G.M.T. in Long. 116° 41′ W., Lat. 17° 55′ N.

Central Eclipse ends May 28^{d} 4^{h} $33^{m}\cdot 3$ G.M.T. in Long. 31° 36′ E., Lat. 25° 24′ N.

Details of Partial Eclipse for British Stations. The times are Greenwich Mean in all cases:—

Station.	Eclipse begins.	Greatest Eclipse.	Eclipse ends.	Magnitude.
	h m	h m	h m	
Greenwich	2 47·0	3 54·9	4 57·5	0·681
Cambridge	2 46·7	3 53·9	4 56·0	0·664
Oxford	2 45·3	3 53·6	4 56·7	0·683
Liverpool	2 42·1	3 49·9	4 52·9	0·656
Edinburgh	2 40·8	3 46·1	4 47·4	0·599
Dublin	2 37·9	3 47·4	4 52·0	0·676

At Greenwich (and throughout the British Isles approximately) first contact is 108° from N. point towards W., and last contact 111° from N. towards E. (direct image).

II.—*A Partial Eclipse of the Moon*, 1900, *June* 12.

Partly visible at Greenwich. The Eclipsed portion of the Moon is very small. Magnitude of the Eclipse ·001 (Moon's diam. = 1). The first contact with the shadow occurs at 176° from the North point towards the East. Last contact at 180°.

	h m		h m
First contact with Penumbra	13 16·2	With shadow	15 24·2
Last " " "	17 39·0	" "	15 31·0

The Moon will set at Greenwich at 15^{h} 54^{m}.

III.—*An Annular Eclipse of the Sun*, 1900, *Nov.* 21.

Invisible at Greenwich. The central line passes from the Loanda Coast, through Rhodesia, and crosses the Indian Ocean, just south of Madagascar. Its course ends in Northern Australia after passing through the Colony of West Australia.

Central Eclipse begins generally November 21^{d} 17^{h} $26^{m}\cdot 9$ G.M.T. in Longitude 2° 30′ E. of Greenwich, Lat. 6° 1′ S. Ends generally November 21^{d} 21^{h} $12^{m}\cdot 8$ in Long. 135° 20′ East of Greenwich, Lat. 18° 31′ S.

OCCULTATIONS, 1900. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap.		Reap.	
			M. T.	P.	M. T.	P.
Jan.			h m	°	h m	°
6	19 Piscium......	5·2	7 12	91	8 10	215
9	27 Arietis	6·5	9 16	139	9 48	191
10	τ^2 Arietis	5·2	7 1	33	7 59	295
10	65 Arietis	5·6	7 55	45	9 1	288
10	D.M. + 20° 602.	6·2	15 10	108	15 58	246
11	κ^1 Tauri	4·6	10 27	115	11 34	241
11	κ^2 Tauri	5·5	10 40	143	11 23	213
13	B.A.C. 1970 ...	6·5	6 32	24	6 59	336
16	α Cancri	4·3	17 17	174	17 50	236
19	p^2 Leonis	6·2	10 0	110	11 6	301
20	B.A.C. 4006 ...	5·7	12 46	157	13 46	263
21	q Virginis	5·7	11 22	122	12 26	291
22	75 Virginis......	5·7	20 33	174	21 5	225
23	B.A.C. 4722 ...	5·5	15 35	136	16 45	273
Feb.						
2	κ Piscium	5·0	6 56	101	7 43	210
6	δ Arietis	4·5	8 36	127	9 24	214
7	A^2 Tauri.........	6·5	7 3	39	8 0	306
7	56 Tauri	5·4	14 3	118	14 49	243
8	n Tauri	5·2	15 50	133	16 29†	237
11	B.A.C. 2649 ...	6·3	17 11	120	18 4‡	276
15	55 Leonis	6·0	13 20	111	14 40	312
Mar.						
5	40 Arietis	6·1	6 22	127	7 7	208
6	B.A.C. 1143 ...	5·9	4 56	84	6 13	256
8	**Neptune.**		6 13	101	7 34	268
10	f Geminorum...	5·2	13 43	98	14 40	299
11	D.M. + 16° 1679	6·5	7 36	48	8 14	353
11	29 Cancri	5·9	14 13	139	15 6	264
15	e Leonis	5·1	15 20	96	16 25	315
21	ρ Ophiuchi (S *)	5·3	17 19	174	17 33	194
23	**Saturn.**		20 35	77	21 46	255
Apr.						
3	B.A.C. 1373 ...	5·7	8 47	88	9 44	275
4	o Tauri	4·8	9 30	71	10 22	304
7	5 Cancri.........	6·4	5 36	82	6 48	317
8	α Cancri.........	4·3	11 46	91	12 45	317
18	24 Ophiuchi ...	5·6	10 36†	85	11 42	293
19	B.A.C. 6088 ...	6·0	15 9	77	16 28	272
20	33 Sagittarii ...	6·0	12 37†	125	13 30	226
20	ξ^2 Sagittarii ...	3·5	14 27	87	15 42	255
23	c^1 Capricorni...	5·2	15 23	11	15 57	306
23	c^2 Capricorni...	6·2	15 34	83	16 37	233
24	κ Aquarii	5·5	14 20†	89	15 12	228
May						
1	ι Tauri	4·7	8 58	110	9 48	259
5	A^1 Cancri	5·6	11 48	124	12 42	277
6	ω Leonis.........	5·6	11 1	79	11 51	329
7	19 Sextantis ...	6·0	10 43	127	11 49	285
15	15 Ophiuchi ...	6·5	12 39	88	13 59	279
18	B.A.C. 6707 ...	6·2	12 39	22	13 18	315
June			h m	°	h	
2	κ Cancri	5·0	8 33	120	9	
7	D.M. − 10° 3570	6·0	9 58	156	10	
13	**Saturn.**		9 40	89	10	
July						
3	B.A.C. 4006 ...	5·7	8 0	111	9	
8	δ Scorpii.........	2·5	11 24	160	11	
9	24 Ophiuchi ...	5·6	10 50	68	12	
10	B.A.C. 6088 ...	6·0	14 17	128	14	
11	33 Sagittarii ...	6·0	10 18	126	11	
11	ξ^2 Sagittarii ...	3·5	12 19	110	13	
14	c^1 Capricorni...	5·2	9 43	13	10	
14	c^2 Capricorni...	6·2	9 54	83	10	
15	κ Aquarii	5·5	8 29†	86	9	
16	16 Piscium......	5·6	11 3	16	11	
16	19 Piscium......	5·2	17 1	359	17	
21	53 Tauri.........	5·5	13 2	63	13	
21	D.M. + 20° 751.	5·9	14 53	150	15	
Aug.						
9	D.M. − 16° 5609	6·0	11 34	89	12	
12	κ Piscium	5·0	16 10	39	17	
16	54 Arietis	6·5	13 1	32	13	
17	B.A.C. 1242 ...	6·6	10 50	131	11	
18	ι Tauri	4·7	12 42	41	13	
18	105 Tauri	5·8	14 47	46	15	
Sept.						
3	**Saturn.**		7 16	128	8	
4	ξ^1 Sagittarii ...	5·0	7 35	62	8	
7	c^2 Capricorni...	6·2	5 41	74	6	
12	π Arietis	5·6	12 35	80	13	
12	ρ^3 Arietis	5·5	16 27	76	17	
13	13 Tauri.........	5·4	9 43	59	10	
13	14 Tauri.........	6·4	10 14	83	11	
14	D.M. + 20° 785.	5·8	8 39†	120	9	
15	ζ Tauri	3·0	9 27†	37	9	
18	29 Cancri	5·9	14 48	69	15	
Oct.						
1	B.A.C. 6347 ...	5·0	8 32	82	9	
2	B.A.C. 6710 ...	6·0	10 8	31	10	
3	B.A.C. 7063 ...	5·2	8 54	64	10	
6	κ Piscium	5·0	13 35	38	14	
8	B.A.C. 325......	6·5	5 52	119	6	
8	D.M. + 10° 168.	6·5	12 9	71	13	
9	19 Arietis	6·2	8 10	18	8	
11	ω^2 Tauri.........	4·6	8 47	33	9	
11	B.A.C. 1342 ...	6·5	10 59	27	11	
11	D.M. + 20° 785.	5·8	18 59	144	19	
12	ζ Tauri	3·0	18 36	51	19	
13	ν Geminorum..	4·0	15 0	40	15	
16	κ Cancri.........	5·0	17 26	149	18	
29	d Sagittarii.....	4·9	8 27	358	8	

* Star rising. ‡ Star setting. † Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., i. e. from
bottom of the Moon's inverted image towards the right.

Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.	
		M.T.	P.	M.T.	P.				M.T.	P.	M.T.	
		h m	°	h m	°	**Dec.**			h m	°	h m	
π Arietis	5·6	9 54	49	10 58	274	2	D.M. + 10° 168 .	6·5	8 18	80	9 28	2
ρ³ Arietis	5·5	13 47	29	14 31	307	3	19 Arietis	6·2	5 7	31	5 55	2
14 Tauri.........	6·4	6 41	13	7 4	320	4	13 Tauri.........	5·4	18 41†	0	18 45†	3
B.A.C. 1242 ...	6·6	14 29	86	15 38	265	5	ω² Tauri	4·6	6 11	26	6 46	3
ι Cancri.........	5·9	14 38	112	15 56	281	5	B.A.C. 1342 ...	6·5	8 32	19	9 5	3
A¹ Cancri	5·6	11 54	100	12 58	289	5	D.M. + 20° 785 .	5·8	16 5	109	17 0	2
A² Cancri	5·8	14 45	190	14 56	207	8	D.M. + 17° 596 .	5·6	13 32	139	14 37	2
ω Leonis	5·6	10 33†	77	11 22	314	10	κ Cancri	5·0	8 56	111	9 52	2
ρ² Leonis	6·2	13 0†	106	14 0	296	11	19 Sextantis ...	6·0	19 59	105	21 5	3
ε Piscium	5·0	6 11	21	7 7	281	12	55 Leonis	6·0	18 5	60	18 46	3
16 Piscium......	5·6	11 23	96	12 14	220	24	B.A.C. 7063 ...	6·2	3 38	101	4 36	2
						26	51 Aquarii	5·8	7 42	29	8 33	2
						30	19 Arietis	6·2	14 3	64	14 53‡	2

† Star below horizon. ‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Feb.			h m	°	Oct.			h m	°
15	ρ² Leonis.	6·2	19 7	23	11	D.M. + 20° 751	5·9	11 54	349
17	B.A.C. 4201 ...	6·3	16 35	26	16	α Cancri	4·3	12 17	13
May.									
8	55 Leonis	6·0	9 52	29	Nov.				
10	B.A.C. 4201 ...	6·3	12 19	23	7	13 Tauri.........	5·4	6 21	347
17	B.A.C. 6336 ...	6·3	12 5	355					
July.					Dec.				
11	ξ¹ Sagittarii ...	5·0	12 53	345	6	ζ Tauri	3·0	15 27	8
Aug.					31	π Arietis	5·6	3 42	341
9	B.A.C. 7063 ...	6·2	13 47	336	31	ρ³ Arietis	5·5	7 22	342
Oct.									
1	D.M. − 20° 5189	6·5	7 20	348					

South Near Approaches.

Jan.			h m	°	Sept.			h m	°
15	5 Cancri.........	6·4	11 44	200	9	22 Piscium ...	5·9	12 5	151
23	D.M. − 17° 4053	6·5	17 29	204	17	D.M. + 17° 1596	5·6	12 11	186
23	B.A.C. 4739 ...	6·2	18 31	202	Oct.				
Feb.					1	B.A.C 6336 ...	6·3	8 24	165
7	51 Tauri	5·4	13 57	181	2	B.A.C. 6707 ...	6·2	9 49	160
16	ε Leonis	5·1	7 51	203	10	B.A.C. 1064 ...	6·5	14 19	168
Apr.					Nov.				
5	ν Geminorum ..	4·0	13 6	192	5	π Piscium	5·6	4 50	159
12	B.A.C. 4006 ...	5·7	8 19	209	9	χ¹ Orionis	4·7	9 53	178
13	η Virginis	5·7	6 45	206	9	χ² Orionis	4·8	14 51	186
17	σ Scorpii	2·5	10 45	195	9	68 Orionis	5·6	19 9	192
July.					Dec.				
21	B.A.C. 1342 ...	6·5	14 31	169	4	B.A.C. 1064 ...	6·5	12 10	171
Aug.									
15	*19 Arietis*	*6·2*	*14 19*	*157*					

ILLUMINATED DISK OF MERCURY.

At Greenwich Noon.

Date.	*k*	*θ*	Date.	*k*	*θ*	Date.	*k*	*θ*
		°			°			°
Jan. 1...	0·776	187·1	May 1...	0·595	151·3	Sept. 8...	0·983	223·2
11...	·884	89·8	11...	·782	153·3	18...	·991	5·6
21...	·944	27·4	21...	·919	159·4	28...	·949	21·6
31...	·983	149·2	31...	·997	323·0	Oct. 8...	·889	23·6
Feb. 10...	·998	54·5	June 10...	·859	352·9	18...	·804	22·2
20...	·947	345·7	20...	·657	4·0	28...	·664	19·4
Mar. 2...	·704	334·6	30...	·479	12·3	Nov. 7...	·396	16·9
12...	·281	327·7	July 10...	·310	19·2	17...	·036	15·0
22...	·020	299·5	20...	·135	28·2	27...	·193	204·4
April 1...	·072	163·3	30...	·015	74·7	Dec. 7...	·607	200·3
11...	·266	154·4	Aug. 9..	·100	177·2	17...	·824	103·3
21...	0·444	151·8	19...	·419	192·3	27...	0·921	183·6
			29...	0·804	203·9			

k is the ratio of the illuminated portion of the apparent disk to the entire apparent disk considered as the superficies of a circle.

θ is the angle which the line joining the cusps, or extremities of the illuminated portion makes with the meridian.

Apparent Disks of Venus and Mars, 1900.

	Venus.	*Mars.*		*Venus.*	*Mars.*		*Venus.*	*Mars.*
Jan. 15...	0·873	1·000	May 15...	0·402	0·975	Sept. 15...	0·493	0·915
Feb. 14...	0·801	0·998	June 15...	0·144	0·962	Oct. 15...	0·637	0·902
Mar. 15...	0·708	0·993	July 15...	0·018	0·948	Nov. 15...	0·751	0·895
April 15...	0·576	0·985	Aug. 15...	0·280	0·932	Dec. 15...	0·836	0·907

The numbers in this table are the versed sines of the illuminated disk, the apparent diameter of the planets being taken as unity.

OPPOSITION OF (433) EROS.

This planet will approach the Earth in December 1900 within a distance of 0·315 (Sun's distance = 1), a closer approach than any other till the year 1931.

At Greenwich Midnight.

	R.A. h m s	N. Dec. ° ′	Log. Radius-vector.	Log. distance from the Earth.	Stellar Magnitude.
1900, Nov. 10...	1 56 24	54 23	0·1145	9·5737	9·38
18...	1 41 8	53 37	·1062	·5484	
26...	1 30 40	51 52	·0982	·5292	9·08
Dec. 4...	1 26 32	49 26	·0904	·5143	
12...	1 29 22	46 23	·0831	·5052	8·88
20...	1 38 48	43 5	·0762	·5000	
28...	1 54 28	39 39	·0701	·4989	8·78
1901, Jan. 5...	2 14 34	36 12	·0649	·5019	
13...	2 38 6	32 45	0·0606	9·5077	8·78

The magnitude will actually be somewhat fainter than that here given, owing to the effect of phase, which is very appreciable. The perihelion passage occurs 1901, Feb. 7^d·84, G.M.T., and the *nearest approach* to the Earth on 1900, Dec. 26.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day	Jan. 18^{h}	Feb. 17^{h}	March. 16^{h}	April. 14^{h}	May. 12^{h}	June. 11^{h}	July. 10^{h}	Aug. 9^{h}	Sept. $7\frac{1}{2}^{h}$	Oct. 6^{h}	Nov. 5^{h}	Day.
1	1O324	42O13	42O3●	3O124	32O14	12O34	412O3	432O1	431O2	21O34	♃O134	1
2	O1234	14O32	421O3	32O4●	31O24	2O134	4O132	4312O	42O13	O2143	213O4	2
3	21O34	3O142	43O12	3421O	♃O324	O234●	312O4	43O12	421O3	14O23	3O214	3
4	2O134	321O4	♃431O	43O12	2O134	13O24	♃32O4	42O3●	♃4O23	423O1	31O24	4
5	31O24	32O14	432O1	41O23	12O43	32O14	3O124	21O43	♃42O1	4321O	23O14	5
6	♃ ♃3O4	O324●	413O2	42O13	4O312	312O4	13O24	O1243	4321O	431O2	24O3●	6
7	32O14	1O234	4O132	41O23	431O2	3O142	2O134	1O324	34O21	♃43O1	41O23	7
8	14O32	2O134	21O43	43O12	432O1	♃14O3	12O34	32O14	31O42	421O3	4O213	8
9	4O123	1O234	♃2O43	342O●	431O2	42O13	O1324	312O4	2O314	4O213	4213O	9
10	421O3	3O142	3O124	3241O	4O312	41O23	♃31O4	3O124	21O34	41O23	43O21	10
11	42O13	3214O	31O24	3O142	42O3●	41O32	♃324O	♃1O34	O1234	♃24O1	431O2	11
12	431O2	432O1	32O14	1O234	421O3	432O1	34O2●	21O34	♃O34●	321O4	423O1	12
13	43O12	4O32●	31O24	2O134	4O132	4321O	413O2	O4213	231O4	♃3O24	42O13	13
14	432O1	♃4O23	O1324	1O234	31O24	43O12	42O13	41O32	3O214	3O124	1O423	14
15	413O2	42O13	21O43	3O124	32O14	41O23	412O3	423O1	31O24	21O34	O2143	15
16	4O123	41O3●	2O143	321O4	31O24	2O413	4O123	4312O	24O1●	O134●	213O4	16
17	12O43	43O12	4O32●	♃32O4	O124●	1O243	413O2	43O12	421O3	1O234		17
18	2O134	3412O	431O2	3O142	2O34●	♃O324	342O1	413O2	4O123	2O314		18
19	13O24	324O1	432O1	14O23	21O34	32O14	34O●●	♃42O3	41O23	321O4		19
20	3O124	1O24●	431O●	42O13	O1234	321O4	31O42	4O13●	4231O	34O12		20
21	32O4●	♃O234	4O312	41O3●	13O24	3O124	2O134	41O32	43O1●	43O2●		21
22	31O4●	2O134	412O3	43O12	32O41	1O324	21O34	23O41	431O2	421O3		22
23	O1324	1O34●	42O13	4312O	341O●	2O143	O1234	321O4	42O1●	4O13●		23
24	12O43	3O124	4O32●	432O1	43O12	1O43●	♃1O24	3O124	214O3	41O23		24
25	24O13	312O4	31O42	43O2●	421O3	4O132	32O14	13O24	O1423	42O31		25
26	413O2	32O14	32O14	41O23	♃42O3	432O●	31O4●	2O134	1O234	4231O		26
27	43O12	1O42●	31O4●	2O413	4O123	4321O	♃3O42	O34●●	♃23O4	34O12		27
28	4321O	4O123	O3124	1O43●	413O2	43O12	42O13	1O234	3O14●	31O42		28
29	♃43O●		12O34	O3124	342O1	41O2●	421O3	23O14	31O24	♃2O34		29
30	4O132		2O134	312O4	3142O	42O13	4O123	321O4	23O14	2O134		30
31	412O3		1O234		3O142		41O32	34O12		1O234		31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

Ephemeris of Jupiter's Satellite V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of 11^{h}·96 to these.

	d	h		d	h		d	h		d	h
March	8	16·5	April	27	12·0	June	16	7·4	July	26	3·9
	18	15·7	May	7	11·1		26	6·5	Aug.	5	3·1
	28	14·8		17	10·2	July	6	5·5		15	2·2
April	7	13·9		27	9·3		16	4·8		25	1·4
	17	13·0	June	6	8·3						

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 40·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

JUPITER'S SATELLITES, 1900.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re. *t*, Tr. Ing.; T, Tr. Eg

January.

d	h	m	
1	7	42	ii. *e*.
	7	56	iii. *e*.
	8	36	i. *t*.
	9	24	iii. E.
	10	49	i. T.
	10	58	iii. *o*.
	11	37	ii. O.
	12	49	iii. O.
2	5	5	i. *e*.
	8	4	i. O.
3	3	6	i. *t*.
	3	52	ii. *t*.
	5	19	i. T.
	6	17	ii. T.
	23	34	i. *e*.
4	2	34	i. O.
	20	58	ii. *e*.
	21	36	i. *t*.
	23	48	i. T.
5	0	58	iii. *t*.
	0	59	ii. O.
	2	49	iii. T.
	18	2	i. *e*.
	21	4	i. O.
6	16	6	i. *t*.
	17	15	ii. *t*.
	18	18	i. T.
	19	40	ii. T.
7	12	31	i. *e*.
	15	34	i. O.
8	10	15	ii. *e*.
	10	35	i. *t*.
	11	54	iii. *e*.
	12	48	i. T.
	13	23	iii. E.
	14	21	ii. O.
	15	19	ii. *o*.
	17	9	iii. O.
9	6	59	i. *e*.
	10	3	i. O.
10	5	5	i. *t*.
	6	39	ii. *t*.
	7	17	i. T.
	9	4	ii. T.
11	1	28	i. *e*.
	4	33	i. O.
	23	31	ii. *e*.
	23	34	i. *t*.
12	1	47	i. T.
	3	43	ii. O.
	5	19	iii. *t*.
	7	9	iii. T.
	19	56	i. *e*.
	23	3	i. O.

Jan. (con.).

d	h	m	
13	18	4	i. *t*.
	20	2	ii. T.
	20	17	i. T.
	22	27	ii. T.
14	14	24	i. *e*.
	17	32	i. O.
15	12	33	i. *t*.
	12	48	ii. *e*.
	14	46	i. T.
	15	51	iii. *e*.
	17	4	ii. O.
	17	21	iii. E.
	19	38	iii. *o*.
	21	28	iii. O
16	8	53	i. *e*.
	12	2	i. O.
17	7	3	i. *t*.
	9	16	i. T.
	9	25	ii. *t*.
	11	50	ii. T.
18	3	21	i. *e*.
	6	32	i. O.
19	1	32	i. *t*.
	2	4	ii. *e*.
	3	45	i. T.
	6	26	ii. O.
	9	37	iii. *t*.
	11	26	iii. T.
	21	50	i. *e*.
20	1	1	i. O.
	20	2	i. *t*.
	22	14	i. T.
	22	47	ii. *t*.
21	1	12	ii. T.
	16	18	i. *e*.
	19	31	i. O.
22	14	31	i. *t*.
	15	21	ii. *e*.
	16	44	i. T.
	19	47	ii. O.
	19	49	iii. *e*.
	21	19	iii. E.
	23	56	iii. *o*.
23	1	44	iii. O
	10	46	i. *e*.
	14	0	i. O.
24	9	1	i. *t*.
	11	13	i. T.
	12	10	ii. *t*.
	14	35	ii. T.
25	5	15	i. *e*.
	8	30	i. O.
26	3	30	i. *t*.
	4	37	ii. *e*.

Jan. (con.).

d	h	m	
26	5	42	i. T.
	9	7	ii. O.
	13	52	iii. *t*.
	15	40	iii. T.
	23	43	i. *e*.
27	2	59	i. O.
	21	59	i. *t*.
28	0	11	i. T.
	1	32	ii. *t*.
	3	57	ii. T.
	18	12	i. *e*.
	21	28	i. O.
29	16	28	i. *t*.
	17	53	ii. *e*.
	18	41	i. T.
	22	28	ii. O.
	23	47	iii. *e*.
30	1	17	iii. E.
	4	11	iii. *o*.
	5	58	iii. O.
	12	40	i. *e*.
	15	58	i. O.
31	10	57	i. *t*.
	13	10	i. T.
	14	54	ii. *t*.
	17	19	ii. T.

February.

d	h	m	
1	7	8	i. *e*.
	10	27	i. O.
2	5	26	i. *t*.
	7	10	ii. *e*.
	7	39	i. T.
	11	48	ii. O.
	18	5	iii. *t*.
	19	52	iii. T.
3	1	37	i. *e*.
	4	56	i. O.
	23	56	i. *t*.
4	2	8	i. T.
	4	14	ii. *t*.
	6	39	ii. T.
	20	5	i. *e*.
	23	25	i. O.
5	18	25	i. *t*.
	20	26	ii. *e*.
	20	37	i. T.
6	1	7	ii. O.
	3	44	iii. *e*.
	5	16	iii. E.
	8	23	iii. *o*.
	10	9	iii. O.
	14	34	i. *e*.

Feb. (con.).

d	h	m	
6	17	55	i. O.
7	12	53	i. *t*.
	15	6	i. T.
	17	36	ii. *t*.
	20	1	ii. T.
8	9	2	i. *e*.
	12	24	i. O.
9	7	22	i. *t*.
	9	35	i. T.
	9	43	ii. *e*.
	14	27	ii. O.
	22	14	iii. *t*.
10	0	0	iii. T.
	3	30	i. *e*.
	6	53	i. O.
11	1	51	i. *t*.
	4	4	i. T.
	6	56	ii. *t*.
	9	21	ii. T.
	21	59	i. *e*.
12	1	21	i. O.
	20	20	i. *t*.
	22	32	i. T.
	22	59	ii. *e*.
13	1	18	ii. E.
	1	20	ii. *o*.
	3	45	ii. O.
	7	42	iii. *e*.
	9	14	iii. E.
	12	31	iii. *o*.
	14	16	iii. O.
	16	27	i. *e*.
	19	50	i. O.
14	14	49	i. *t*.
	17	1	i. T.
	20	16	ii. *t*.
	22	41	ii. T.
15	10	55	i. *e*.
	14	19	i. O.
16	9	17	i. *t*.
	11	30	i. T.
	12	16	ii. *e*.
	14	35	ii. E.
	14	39	ii. *o*.
	17	4	ii. O.
17	2	20	iii. *t*.
	4	5	iii. T.
	5	24	i. *e*.
	8	48	i. O.
18	3	46	i. *t*.
	5	58	i. T.
	9	35	ii. *t*.
	12	0	ii. T.
	23	52	i. *e*.

Feb. (con.).

d	h	m	
19	3	17	i. O.
	22	14	i. *t*.
20	0	27	i. T.
	1	33	ii. *e*.
	3	51	ii. E.
	3	57	ii. *o*.
	6	22	ii. O.
	11	39	iii. *e*.
	13	12	iii. E.
	16	36	iii. *o*.
	18	20	iii. O.
	18	21	i. *e*.
	21	45	i. O.
21	16	43	i. *t*.
	18	55	i. T.
	22	54	ii. *t*.
22	1	20	ii. T.
	12	49	i. *e*.
	16	14	i. O.
23	11	11	i. *t*.
	13	24	i. T.
	14	49	ii. *e*.
	17	8	ii. E.
	17	14	ii. *o*.
	19	40	ii. O.
24	6	24	iii. *t*.
	7	17	i. *e*.
	8	7	iii. T.
	10	42	i. O.
25	5	40	i. *t*.
	7	52	i. T.
	12	13	ii. *t*.
	14	38	ii. T.
26	1	46	i. *e*.
	5	11	i. O.
27	0	8	i. *t*.
	2	21	i. T.
	4	5	ii. *e*.
	6	25	ii. E.
	6	32	ii. *o*.
	8	57	ii. O.
	15	37	iii. *e*.
	17	10	iii. E.
	20	14	i. *e*.
	20	37	iii. *o*.
	21	20	iii. O.
	23	39	i. O.
28	18	37	i. *t*.
	20	49	i. T.

March.

d	h	m	
1	1	31	ii. *t*.
	3	56	ii. T.

d	
1	
2	
3	
4	
5	
6	
	1
	1
	2
	2
7	
	2
	2
8	
	1
	2
9	1
	1
	1
	2
	2
10	
	1
	1
	1
	1
11	
	1
	1
	1
12	
13	

Mar. (con.).

	h	m	
3	9	11	ii. e.
	11	32	ii. E.
	11	36	ii. o.
	14	1	ii. O.
	23	31	iii. e.
4	0	1	i. e.
	1	6	iii. E.
	3	25	i. O.
	4	26	iii. o.
	6	7	iii. O.
	2	21	i. t.
15	0	33	i. T.
	6	38	ii. t.
	9	3	ii. T.
	18	29	i. e.
	21	53	i. O.
16	16	49	i. t.
	19	1	i. T.
	22	28	ii. e.
17	0	48	ii. E.
	0	51	ii. o.
	3	16	ii. O.
	12	58	i. e.
	16	20	i. O.
	18	7	iii. t.
	19	48	iii. T.
18	11	16	i. t.
	13	29	i. T.
	19	53	ii. t.
	22	18	ii. T.
19	7	26	i. e.
	10	48	i. O.
20	5	44	i. t.
	7	56	i. T.
	11	45	ii. e.
	16	30	ii. O.
21	1	54	i. e.
	3	29	iii. e.
	5	4	iii. E.
	5	16	i. O.
	8	15	iii. o.
	9	55	iii. O.
22	0	12	i. t.
	2	24	i. T.
	9	8	ii. t.
	11	33	ii. T.
	20	23	i. e.
	23	43	i. O.
23	18	39	i. t.
	20	51	i. T.
24	1	2	ii. e.
	5	44	ii. O.
	14	51	i. e.
	18	11	i. O.
	21	53	iii. t.
	23	33	iii. T.
25	13	6	i. t.
	15	19	i. T.
	22	22	ii. t.
26	0	47	ii. T.
	9	19	i. e.
	12	38	i. O.
27	7	34	i. t.
	1	40	i. T.

Mar. (con.).

d	h	m	
27	14	18	ii. e.
	18	57	ii. O.
28	3	48	i. e.
	7	6	i. O.
	7	26	iii. e.
	9	3	iii. E.
	11	59	iii. o.
	13	38	iii. O.
29	2	1	i. t.
	4	13	i. T.
	11	36	ii. t.
	14	1	ii. T.
	22	16	i. e.
30	1	33	i. O.
	20	28	i. t.
	22	40	i. T.
31	3	35	ii. e.
	8	10	ii. O.
	16	44	i. e.
	20	0	i. O.

April.

d	h	m	
1	1	34	iii. t.
	3	13	iii. T.
	14	55	i. t.
	17	8	i. T.
2	0	49	ii. t.
	3	13	ii. T.
	11	13	i. e.
	14	27	i. O.
3	9	23	i. t.
	11	35	i. T.
	16	52	ii. e.
	21	22	ii. O.
4	5	41	i. e.
	8	54	i. O.
	11	24	iii. e.
	13	2	iii. E.
	15	38	iii. o.
	17	17	iii. O.
5	3	50	i. t.
	6	2	i. T.
	14	1	ii. t.
	16	26	ii. T.
6	0	9	i. e.
	3	21	i. O.
	22	17	i. t.
7	0	29	i. T.
	6	9	ii. e.
	10	34	ii. O.
	18	38	i. e.
	21	48	i. O.
8	5	11	iii. t.
	6	49	iii. T.
	16	43	i. t.
	18	56	i. T.
9	3	13	ii. t.
	5	38	ii. T.
	13	6	i. e.
	16	15	i. O.
10	11	10	i. t.
	13	23	i. T.
	22	28	ii. e.

Apr. (con.).

d	h	m	
10	23	45	ii. O.
11	7	35	i. e.
	10	42	i. O.
	15	21	iii. e.
	17	0	iii. E.
	19	13	iii. o.
	20	51	iii. O.
12	5	36	i. t.
	7	48	i. T.
	16	25	ii. t.
	18	49	ii. T.
13	2	3	i. e.
	5	9	i. O.
14	0	4	i. t.
	2	16	i. T.
	8	43	ii. e.
	12	56	ii. O.
	20	31	i. e.
	23	36	i. O.
15	8	45	iii. t.
	10	23	iii. T.
	18	31	i. t.
	20	43	i. T.
16	5	35	ii. t.
	8	0	ii. T.
	15	0	i. e.
	18	2	i. O.
17	12	57	i. t.
	15	9	i. T.
	22	0	ii. e.
18	2	6	ii. O.
	9	28	i. e.
	12	29	i. O.
	19	18	iii. e.
	20	58	iii. E.
	22	43	iii. o.
19	0	20	iii. O.
	7	24	i. t.
	9	36	i. T.
	18	46	ii. t.
	21	10	ii. T.
20	3	57	i. e.
	6	56	i. O.
21	1	50	i. t.
	4	2	i. T.
	11	17	ii. e.
	15	16	ii. O.
	22	25	i. e.
22	1	22	i. O.
	12	12	iii. t.
	13	49	iii. T.
	20	17	i. t.
	22	29	i. T.
23	7	55	ii. t.
	10	20	ii. T.
	16	53	i. e.
	19	48	i. O.
24	14	43	i. t.
	16	55	i. T.
25	0	35	ii. e.
	4	25	ii. O.
	11	22	i. e.
	14	15	i. O.

Apr. (con.).

d	h	m	
26	0	57	iii. E.
	2	9	iii. o.
	3	46	iii. O.
	9	10	i. t.
	11	22	i. T.
	21	5	ii. t.
	23	29	ii. T.
27	5	50	i. e.
	8	41	i. O.
28	3	36	i. t.
	5	48	i. T.
	13	52	ii. e.
	17	35	ii. O.
29	0	19	i. e.
	3	7	i. O.
	15	37	iii. t.
	17	14	iii. T.
	22	2	i. t.
30	0	14	i. T.
	10	14	ii. t.
	12	38	ii. T.
	18	47	i. e.
	21	34	i. O.

May.

d	h	m	
1	16	28	i. t.
	18	40	i. T.
2	3	9	ii. e.
	6	43	ii. O.
	13	15	i. e.
	16	0	i. O.
3	3	13	iii. e.
	4	55	iii. E.
	5	31	iii. o.
	7	8	iii O.
	10	55	i. t.
	13	7	i. T.
	23	22	ii. t.
4	1	46	ii. T.
	7	44	i. e.
	10	26	i. O.
5	5	21	i. t.
	7	33	i. T.
	16	27	ii. e.
	19	52	ii. O.
6	2	12	i. e.
	4	52	i. O.
	18	58	iii. t.
	20	35	iii. T.
	23	47	i. t.
7	1	59	i. T.
	12	30	ii. t.
	14	54	ii. T.
	20	41	i. e.
	23	18	i. O.
8	18	13	i. t.
	20	25	i. T.
9	5	44	ii. e.
	9	0	ii. O.
	15	9	i. e.
	17	44	i. O.
10	7	11	iii. e.
	10	29	iii. O.

May (con.).

d	h	m	
10	12	39	i. t.
	14	51	i. T.
11	1	38	ii. t.
	4	2	ii. T.
	9	38	i. e.
	12	10	i. O.
12	7	5	i. t.
	9	17	i. T.
	19	2	ii. e.
	22	8	ii. O.
13	4	6	i. e.
	6	36	i. O.
	22	27	iii. t.
	23	55	iii. T.
14	1	31	i. t.
	3	43	i. T.
	14	45	ii. t.
	17	9	ii. T.
	22	34	i. e.
15	1	2	i. O.
	19	57	i. t.
	22	9	i. T.
16	8	20	ii. e.
	11	15	ii. O.
	17	3	i. e.
	19	28	i. O.
17	11	9	iii. e.
	13	48	iii. O.
	14	23	i. t.
	16	35	i. T.
18	3	52	ii. t.
	6	16	ii. T.
	11	31	i. e.
	13	54	i. O.
19	8	49	i. t.
	11	1	i. T.
	21	37	ii. e.
20	0	23	ii. O.
	6	0	i. e.
	8	20	i. O.
21	1	33	iii. t.
	3	13	iii. T.
	3	15	i. t.
	5	27	i. T.
	16	59	ii. t.
	19	23	ii. T.
22	0	28	i. e.
	2	46	i. O.
	21	41	i. t.
	23	53	i. T.
23	10	55	ii. e.
	13	31	ii. O.
	18	57	i. e.
	21	12	i. O.
24	15	7	iii. e.
	16	7	i. t.
	17	6	iii. O.
	18	9	i. T.
25	6	6	ii. t.
	8	30	ii. T.
	13	25	i. e.
	15	38	i. O.
26	10	33	i. t.
	12	45	i. T.

May (con.).

d	h	m	
27	0	13	ii. e.
	2	38	ii. O
	7	52	i. o.
	10	4	i. O.
28	4	49	iii. t
	4	59	i. t.
	6	30	iii. T
	7	11	i. T.
	19	12	ii. t.
	21	37	ii. T
29	2	18	i. o.
	4	31	i. E.
	23	25	i. t.
30	1	37	i. T.
	13	21	ii. o.
	15	55	ii. E
	20	44	i. o.
	22	59	i. E.
31	17	51	i. t.
	18	41	iii. o
	20	3	i. T.
	20	52	iii. E

June.

d	h	m	
1	8	19	ii. t.
	10	44	ii. T
	15	9	i. o.
	17	28	i. E.
2	12	17	i. t.
	14	29	i. T.
3	2	28	ii. o.
	5	13	ii. E
	9	35	i. o.
	11	56	i. E.
4	6	43	i. t.
	8	5	iii. t
	8	55	i. T.
	9	49	iii. T
	21	26	ii. t.
	23	51	ii. T
5	4	1	i. o.
	6	25	i. E.
6	1	9	i. t.
	3	21	i. T.
	15	36	ii. o.
	18	32	ii. E
	22	27	i. o.
7	0	53	i. E.
	19	35	i. t.
	21	48	i. T.
	21	57	iii. o
8	0	51	iii. E
	10	33	ii. t.
	12	58	ii. T
	16	53	i. o.
	19	22	i. E.
9	14	1	i. t.
	16	14	i. T.
10	4	44	ii. o.
	7	50	ii.
	11	19	i.
	13	50	i.

June (con.).

d	h	m	
11	8	27	i. *t*.
	10	40	i. T.
	11	23	iii. *t*.
	13	8	iii. T.
	23	40	ii. *t*.
12	2	5	ii. T.
	5	45	i. *o*.
	8	19	i. E.
13	2	54	i *t*.
	5	6	i. T.
	17	52	ii. *o*.
	21	8	ii. E.
14	0	12	i. *o*.
	2	47	i. E.
	21	20	i. *t*.
	23	32	i. T.
15	1	14	iii. *o*.
	3	1	iii. O.
	3	1	iii. *e*.
	4	50	iii. E.
	12	47	ii. *t*.
	15	12	ii. T.
	18	38	i. *o*.
	21	16	i. E.
16	15	46	i. *t*.
	17	59	i. T.
17	7	1	ii. *o*.
	10	26	i. E.
	13	4	i. *o*.
	15	45	i. E.
18	10	13	i. *t*.
	12	25	i. T.
	14	42	iii. *t*.
	16	30	iii. T.
19	1	55	ii. *t*.
	4	20	ii. T.
	7	30	i. *o*.
	10	13	i. E.
20	4	39	i. *t*.
	6	51	i. T.
	20	10	ii. *o*.
	23	45	ii. E.
21	1	57	i. *o*.
	4	42	i. E.
	23	6	i. *t*.
22	1	18	i. T.
	4	33	iii. *o*.
	6	23	iii. O.
	7	0	iii. *e*.
	8	49	iii. E.
	15	3	ii. *t*.
	17	28	ii. T.
	20	23	i. *o*.
	23	10	i. E.
23	17	32	i. *t*.
	19	45	i. T.
24	9	19	ii. *o*.
	13	3	ii. E.
	14	49	i. *o*.
	17	39	i. E.
25	11	59	i. *t*.
	14	11	i. T.
	18	3	iii. *t*.
	19	54	iii. T.

June (con.).

d	h	m	
26	4	11	ii. *t*.
	6	37	ii. T.
	9	16	i. *o*.
	12	7	i. E.
27	6	25	i. *t*.
	8	38	i. T.
	22	29	ii. *o*.
28	2	22	ii. E.
	3	42	i. *o*.
	6	36	i. E.
29	0	52	i. *t*.
	3	5	i. T.
	7	56	iii. *o*.
	9	49	iii. O.
	10	58	iii. *e*.
	12	49	iii. E.
	17	20	ii. *t*.
	19	46	ii. T.
	22	9	i. *o*.
30	1	5	i. E.
	19	19	i. *t*.
	21	31	i. T.

July.

d	h	m	
1	11	39	ii. *o*.
	15	41	ii. E.
	16	35	i. *o*.
	19	33	i. E.
2	13	45	i. *t*.
	15	58	i. T.
	21	28	iii. *t*.
	23	21	iii. *t*.
3	6	29	ii. *t*.
	8	55	ii. T.
	11	2	i. *o*.
	14	2	i. E.
4	8	12	i. *t*.
	10	25	ii. T.
5	0	49	ii. *o*.
	5	0	ii. E.
	5	29	i. *o*.
	8	31	i. E.
6	2	39	i. *t*.
	4	52	i. T.
	11	23	iii. *o*.
	13	18	iii. O.
	14	57	iii. *e*.
	16	49	iii. E.
	19	38	ii. *t*.
	22	5	ii. T.
	23	56	i. *o*.
7	2	59	i. E.
	21	6	i. *t*.
	23	19	i. T.
8	14	0	ii. *o*.
	18	18	ii. E.
	18	22	i. *o*.
	21	28	i. E.
9	15	33	i. *t*.
	17	46	i. T.
10	0	56	iii. *t*.
	2	52	iii. T.

July (con.).

d	h	m	
10	8	48	ii. *t*.
	11	15	ii. T.
	12	49	i. *o*.
	15	56	i. E.
11	10	0	i. *t*.
	12	13	i. T.
12	3	12	ii. *o*.
	7	16	i. *o*.
	7	37	ii. E.
	10	25	i. E.
13	4	27	i. *t*.
	6	40	i. T.
	14	54	iii. *o*.
	16	51	iii. O.
	18	56	iii. *e*.
	20	49	iii. E.
	21	58	ii. *t*.
14	0	26	ii. T.
	1	43	i. *o*.
	4	54	i. E.
	22	54	i. *t*.
15	1	7	i. T.
	16	24	ii. *o*.
	20	10	i. *o*.
	20	56	ii. E.
	23	22	i. E.
16	17	22	i. *t*.
	19	35	i. T.
17	4	29	iii. *t*.
	6	28	iii. T.
	11	10	ii. *t*.
	13	37	ii. T.
	14	37	i. *o*.
	17	51	i. E.
18	11	49	i. *t*.
	14	2	i. T.
19	5	37	ii. *o*.
	9	5	i. *o*.
	10	15	ii. E.
	12	20	i. E.
20	6	16	i. *t*.
	8	30	i. T.
	18	28	iii. *o*.
	20	28	iii. O.
	22	55	iii. *e*.
21	0	21	ii. *t*.
	0	49	iii. E.
	2	49	ii. T.
	3	32	i. *o*.
	6	48	i. E.
22	0	44	i. *t*.
	2	58	i. T.
	18	50	ii. *o*.
	22	0	i. *o*.
	23	33	ii. E.
23	1	17	i. E.
	19	12	i. *t*.
	21	25	i. T.
24	8	7	iii. *t*.
	10	8	iii. T.
	13	33	ii. *t*.
	16	1	ii. T.
	16	27	i. *o*.
	19	46	i. E.

July (con.).

d	h	m	
25	13	39	i. *t*.
	15	53	i. T.
26	8	4	ii. *o*.
	10	55	i. *o*.
	12	53	ii. E.
	14	14	i. E.
27	8	7	i. *t*.
	10	20	i. T.
	22	7	iii. *o*.
28	0	10	iii. O.
	2	46	ii. *t*.
	2	53	iii. *e*.
	4	49	iii. E.
	5	15	ii. T.
	5	22	i. *o*.
	8	43	i. E.
29	2	35	i. *t*.
	4	48	i. T.
	21	19	ii. *o*.
	23	50	i. *o*.
30	2	11	ii. E.
	3	12	i. E.
	21	3	i. *t*.
	23	16	i. T.
31	11	49	iii. *t*.
	13	53	iii. T.
	16	0	ii. *t*.
	18	18	i. *o*.
	18	28	ii. T.
	21	40	i. E.

August.

d	h	m	
1	15	31	i. *t*.
	17	44	i. T.
2	10	34	ii. *o*.
	12	46	i. *o*.
	13	2	ii. O.
	13	3	ii. *e*.
	15	31	ii. E.
	16	9	i. E.
3	9	59	i. *t*.
	12	12	i. T.
4	1	51	iii. *o*.
	3	56	iii. O.
	5	14	ii. *t*.
	6	52	iii. *e*.
	7	13	i. *o*.
	7	42	ii. T.
	8	49	iii. E.
	10	38	i. E.
5	4	27	i. *t*.
	6	40	i. T.
	23	50	ii. *o*.
6	1	41	i. *o*.
	2	19	ii. O.
	2	21	ii. *e*.
	4	49	ii. E.
	5	7	i. E.
	22	55	i. *t*.
7	1	8	i. T.
	15	36	iii. *t*.
	17	43	iii. T.
	18	28	ii. *t*.

Aug. (con.).

d	h	m	
7	20	9	i. *o*.
	20	57	ii. T.
	23	35	i. E.
8	17	23	i. *t*.
	19	37	i. T.
9	13	6	ii. *o*.
	14	37	i. *o*.
	15	35	ii. O.
	15	40	ii. *e*.
	18	4	i. E.
	18	9	ii. E.
10	11	51	i. *t*.
	14	5	i. T.
11	5	39	iii. *o*.
	7	43	ii. *t*.
	7	47	iii. O.
	9	6	i. *o*.
	10	12	ii. T.
	10	50	iii. *e*.
	12	33	i. E.
	12	49	iii. E.
12	6	20	i. *t*.
	8	34	i. T.
13	2	23	ii. *o*.
	3	34	i. *o*.
	4	52	ii. O.
	4	59	ii. *e*.
	7	1	i. E.
	7	27	ii. E.
14	0	48	i. *t*.
	3	2	i. T.
	19	26	iii. *t*.
	20	59	ii. *t*.
	21	36	iii. T.
	22	2	i. *o*.
	23	28	ii. T.
15	1	30	i. E.
	19	17	i. *t*.
	21	31	i. T.
16	15	40	ii. *o*.
	16	31	i. *o*.
	18	10	ii. O.
	18	18	ii. *e*.
	19	59	i. E.
	20	47	ii. E.
17	13	46	i. *t*.
	15	59	i. T.
18	9	32	iii. *o*.
	10	15	ii. *t*.
	10	59	i. *o*.
	11	42	iii. O.
	12	44	ii. T.
	14	27	i. E.
	14	50	iii. *e*.
	16	49	iii. E.
19	8	14	i. *t*.
	10	28	i. T.
20	4	58	ii. *o*.
	5	28	i. *o*.
	7	28	ii. O.
	7	36	ii. *e*.
	8	56	i. E.
	10	5	ii. E.
21	2	43	i. *t*.

Aug. (c

d	h	m
21	4	57
	23	21
	23	31
	23	56
22	1	33
	2	1
	3	25
	21	12
	23	25
23	18	17
	18	25
	20	47
	20	56
	21	54
	23	25
24	15	41
	17	54
25	12	48
	12	54
	13	29
	15	19
	15	42
	16	22
	18	49
	20	50
26	10	10
	12	23
27	7	22
	7	35
	10	6
	10	14
	10	51
	12	43
28	4	38
	6	52
29	1	51
	2	6
	3	20
	4	37
	5	20
	5	34
	23	7
30	1	21
	20	20
	20	55
	23	26
	23	33
	23	48
31	2	3
	17	36
	19	50

Septem

d	h	m
1	14	49
	15	24
	17	30
	17	55
	18	17
	19	45
	22	48
2	0	50
	12	5
	14	19

on.).
i. *o.*
ii. *o.*
i. E.
ii. O.
ii. *e.*
ii. E.
i. *t.*
i. T.
i. *o.*
ii. *t.*
ii. T.
i. E.
iii. *t.*
iii. T.
i *t.*
i. T.
i. *o.*
ii. *o.*
i. E.
ii. O.
ii. *e.*
ii. E.
i. *t.*
i. T.
i. *o.*
ii. *t.*
i. E.
ii. T.
iii. *o.*
iii. O.
iii. *e.*
iii. E.
i. *t.*
i. T.
i. *o.*
ii. *o.*
i. E.
ii. O.
ii. *e.*
ii. E.
i *t.*
i. T.
i. *o.*
ii. *t.*
i. E.
ii. T.
iii. *t.*
iii. T.
i. *t.*
i. T.
i. *o.*
ii. *o.*
i. E.
ii. E.
i. *t.*

Sept. (con.).

d	h	m	
14	23	45	i. T.
15	18	42	i. *o.*
	20	41	ii. *t.*
	22	7	i. E.
	23	14	ii. T.
16	1	43	iii. *o.*
	4	0	iii. O.
	6	45	iii. *e.*
	8	50	iii. E.
	16	0	i. *t.*
	18	14	i. T.
17	13	11	i. *o.*
	15	38	ii. *o.*
	16	35	i. E.
	20	37	ii. E.
18	10	30	i. *t.*
	12	44	i. T.
19	7	41	i. *o.*
	10	2	ii. *t.*
	11	4	i. E.
	12	34	ii. T.
	15	41	iii. *t.*
	18	0	iii. T.
20	4	59	i. *t.*
	7	13	i. T.
21	2	10	i. *o.*
	5	0	ii. *o.*
	5	33	i. E.
	9	56	ii. E.
	23	29	i. *t.*
22	1	43	i. T.
	20	40	i. *o.*
	23	23	ii. *t.*
23	0	2	i. E.
	1	55	ii. T.
	5	54	iii. *o.*
	8	13	iii. O.
	10	44	iii. *e.*
	12	50	iii. E.
	17	58	i. *t.*
	20	13	i. T.
24	15	9	i. *o.*
	18	22	ii. *o.*
	18	30	i. E.
	23	15	ii. E.
25	12	28	i. *t.*
	14	42	i. T.
26	9	39	i. *o.*
	12	44	ii. *t.*
	12	59	i. E.
	15	17	ii. T.
	19	55	iii. *t.*
	22	15	iii. T.
27	6	58	i. *t.*
	9	12	i. T.

Sept. (con.).

d	h	m	
28	4	9	i. *o.*
	7	28	i. E.
	7	45	ii. *o.*
	12	34	ii. E.
29	1	28	i. *t.*
	3	42	i. T.
	22	38	i. *o.*
30	1	56	i. E.
	2	6	ii. *t.*
	4	39	ii. T.
	10	9	iii. *o.*
	12	29	iii. O.
	14	43	iii. *e.*
	16	50	iii. E.
	19	58	i. *t.*
	22	12	i. T.

October.

d	h	m	
1	17	8	i. *o.*
	20	25	i. E.
	21	8	ii. *o.*
2	1	52	ii. E.
	14	27	i. *t.*
	16	42	i. T.
3	11	38	i. *o.*
	14	54	i. E.
	15	28	ii. *t.*
	18	1	ii. T.
4	0	11	iii. *t.*
	2	32	iii. T.
	8	57	i. *t.*
	11	12	i. T.
5	6	8	i. *o.*
	9	22	i. E.
	10	31	ii. *o.*
	15	12	ii. E.
6	3	27	i. *t.*
	5	42	i. T.
7	0	37	i. *o.*
	3	51	i. E.
	4	50	ii. *t.*
	7	23	ii. T.
	14	26	iii. *o.*
	16	48	iii. O.
	18	42	iii. *e.*
	20	51	iii. E.
	21	57	i. *t.*
8	0	12	i. T.
	19	7	i. *o.*
	22	20	i. E.
	23	55	ii. *o.*
9	4	30	ii. E.
	16	27	i. *t.*
	18	42	i. T.

Oct. (con.).

d	h	m	
10	13	37	i. *o.*
	16	49	i. E.
	18	12	ii. *t.*
	20	46	ii. T.
11	4	29	iii. *t.*
	6	51	iii. T.
	10	57	i. *t.*
	13	12	i. T.
12	8	7	i. *o.*
	11	17	i. E.
	13	18	ii. *o.*
	17	49	ii. E.
13	5	27	i. *t.*
	7	42	i. T.
14	2	37	i. *o.*
	5	46	i. E.
	7	35	ii. *t.*
	10	9	ii. T.
	18	46	iii. *o.*
	21	9	iii. O.
	22	41	iii. *e.*
	23	57	i. *t.*
15	0	51	iii. E.
	2	12	i. T.
	21	7	i. *o.*
16	0	15	i. E.
	2	42	ii. *o.*
	7	7	ii. E.
	18	27	i. *t.*
	20	42	i. T.
17	15	37	i. *o.*
	18	43	i. E.
	20	58	ii. *t.*
	23	32	ii. T.
18	8	50	iii. *t.*
	11	13	iii. T.
	12	57	i. *t.*
	15	12	i. T.
19	10	7	i. *o.*
	13	12	i. E.
	16	6	ii. *o.*
	20	26	ii. E.
20	7	28	i. *t.*
	9	42	i. T.
21	4	37	i. *o.*
	7	41	i. E.
	10	21	ii. *t.*
	12	56	ii. T.
	23	7	iii. *o.*
22	1	3	iii. O.
	1	58	i. *t.*
	2	40	iii. *e.*
	4	12	i. T.
	4	51	iii. E.
	23	7	i. *o.*

Oct. (con.).

d	h	m	
23	2	9	i. E.
	5	30	ii. *o.*
	9	44	ii. E.
	20	28	i. *t.*
	22	43	i. T.
24	17	37	i. *o.*
	20	38	i. E.
	23	45	ii. *t.*
25	2	20	ii. T.
	13	12	iii. *t.*
	14	58	i. *t.*
	15	37	iii. T.
	17	13	i. T.
26	12	8	i. *o.*
	15	7	i. E.
	18	55	ii. *o.*
	23	3	ii. E.
27	9	28	i. *t.*
	11	43	i. T.
28	6	38	i. *o.*
	9	35	i. E.
	13	9	ii. *t.*
	15	44	ii. T.
29	3	30	iii. *o.*
	3	59	i. *t.*
	5	56	iii. O.
	6	13	i. T.
	6	39	iii. *e.*
	8	51	iii. E.
30	1	8	i. *o.*
	4	4	i. E.
	8	19	ii. *o.*
	12	21	ii. E.
	22	29	i. *t.*
31	0	44	i. T.
	19	38	i. *o.*
	22	33	i. E.

November.

d	h	m	
1	2	33	ii. *t.*
	5	8	ii. T.
	16	59	i. *t.*
	17	37	iii. *t.*
	19	14	i. T.
	20	3	iii. T.
2	14	8	i. *o.*
	17	1	i. E.
	21	44	ii. *o.*
3	1	40	ii. E.
	11	29	i. *t.*
	13	44	i. T.
4	8	38	i. *o.*
	11	30	i. E.

Nov. (con.).

d	h	m	
4	15	57	ii. *t.*
	18	32	ii. T.
5	6	0	i. *t.*
	7	55	iii. *o.*
	8	15	i. T.
	10	22	iii. O.
	10	37	iii. *e.*
	12	51	iii. E.
6	3	9	i. *o.*
	5	59	i. E.
	11	9	ii. *o.*
	14	58	ii. E.
7	0	30	i. *t.*
	2	45	i. T.
	21	39	i. *o.*
8	0	27	i. E.
	5	21	ii. *t.*
	7	57	ii. T.
	19	0	i. *t.*
	21	15	i. T.
	22	2	iii. *t.*
9	0	30	iii. T.
	16	9	i. *o.*
	18	56	i. E.
10	0	33	ii. *o.*
	4	17	ii. E.
	13	31	i. *t.*
	15	46	i. T.
11	10	40	i. *o.*
	13	25	i. E.
	18	45	ii. *t.*
	21	22	ii. T.
12	8	1	i. *t.*
	10	16	i. T.
	12	20	iii. *o.*
	16	51	iii. E.
13	5	10	i. *o.*
	7	53	i. E.
	13	58	ii. *o.*
	17	34	ii. E.
14	2	31	i. *t.*
	4	46	i. T.
	23	40	i. *o.*
15	2	22	i. E.
	8	10	ii. *t.*
	10	47	ii. T.
	21	2	i. *t.*
	23	17	i. T.
16	2	29	iii. *t.*
	4	58	iii. T.
	18	11	i. *o.*
	20	51	i. E.

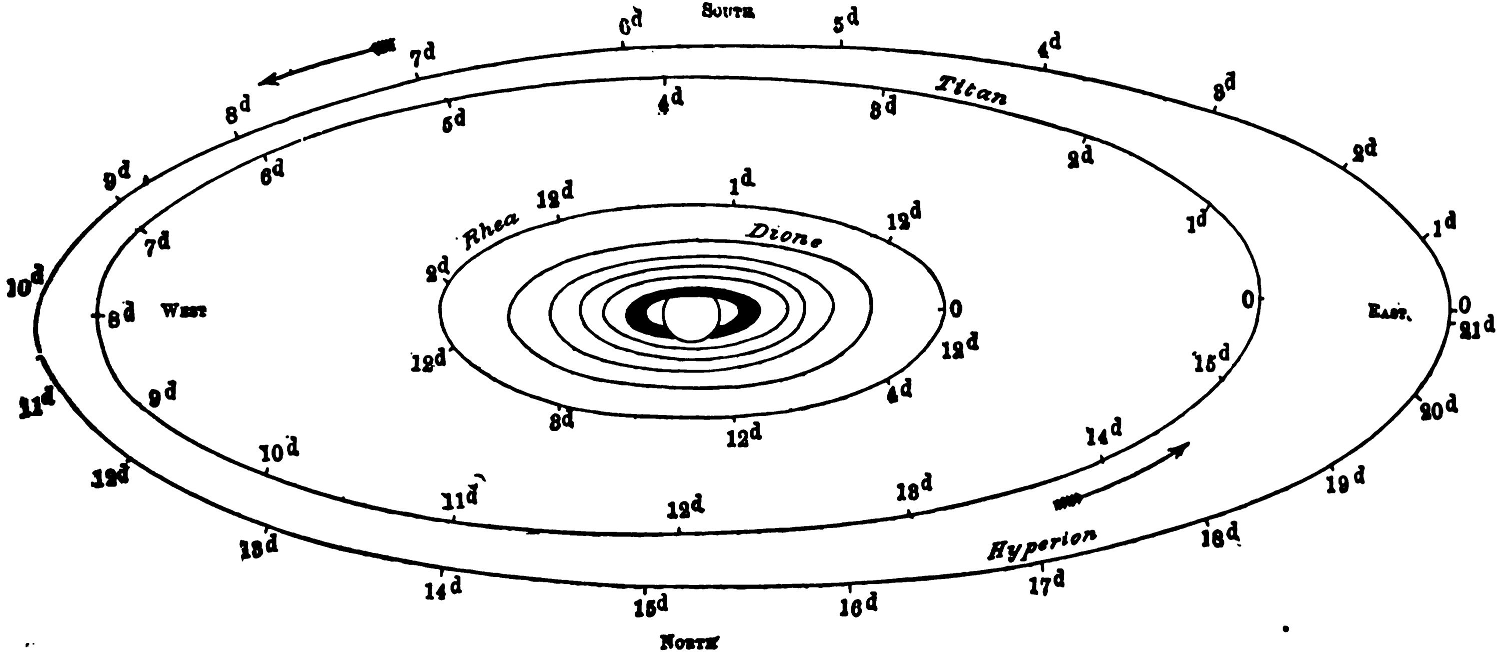
tellites of Jupiter will be invisible from November 17 until 1901, January 10, Jupiter being too
Sun.

tes I. & II. *disappear* at eclipse on the *p* side from January to May; they *reappear* on the
on June to November. Satellite II. *reappears* on the *p* side in February and March, and
s on the *f* side in August.

te III. *disappears* and *reappears* on the *p* side from January to April. It *disappears* on the
May. It *disappears* and *reappears* on the *f* side from June to November.

is no eclipse of *Satellite IV* [illegible] the year.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

.	March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
8·0	1	7·2	1	9·5	1	13·3	1	15·5	1	19·2	1	21·5	1	1·3	1	5·2	1	7·8
6·6	2	5·8	2	8·1	2	11·9	2	14·1	2	17·8	2	20·1	1	23·9	2	3·8	2	6·5
5·2	3	4·4	3	6·8	3	10·5	3	12·8	3	16·4	3	18·8	2	22·5	3	2·4	3	5·1
3·9	4	3·0	4	5·4	4	9·1	4	11·4	4	15·0	4	17·4	3	21·1	4	1·1	4	3·7
2·5	5	1·6	5	4·0	5	7·7	5	10·0	5	13·6	5	16·0	4	19·7	4	23·7	5	2·3
1·1	6	0·3	6	2·6	6	6·4	6	8·6	6	12·3	6	14·6	5	18·4	5	22·3	6	1·0
3·7	6	22·9	7	1·2	7	5·0	7	7·2	7	10·9	7	13·2	6	17·0	6	20·9	6	23·6
2·3	7	21·5	7	23·9	8	3·6	8	5·9	8	9·5	8	11·9	7	15·6	7	19·5	7	22·2
1·0	8	20·1	8	22·5	9	2·2	9	4·5	9	8·1	9	10·5	8	14·2	8	18·2	8	20·8
9·6	9	18·7	9	21·1	10	0·8	10	3·1	10	6·7	10	9·1	9	12·9	9	16·8	9	19·4
8·2	10	17·4	10	19·7	10	23·5	11	1·7	11	5·4	11	7·7	10	11·5	10	15·4	10	18·1
6·8	11	16·0	11	18·3	11	22·1	12	0·3	12	4·0	12	6·3	11	10·2	11	14·0		
5·4	12	14·6	12	17·0	12	20·7	12	22·9	13	2·6	13	4·9	12	8·8	12	12·7		
4·1	13	13·2	13	15·6	13	19·3	13	21·5	14	1·2	14	3·5	13	7·4	13	11·3		
2·7	14	11·8	14	14·2	14	17·9	14	20·1	14	23 8	15	2·1	14	6·0	14	10·0		
1·3	15	10·5	15	12.8	15	16·5	15	18·7	15	22·4	16	0·7	15	4·7	15	8·6		
9·9	16	9·1	16	11·4	16	15·1	16	17·3	16	21·0	16	23·3	16	3·3	16	7·2		
8·5	17	7·7	17	10·1	17	13·7	17	16·0	17	19·6	17	22·0	17	1·9	17	5·8		
	18	6·3	18	8·7	18	12·3	18	14·6	18	18·2	18	20·6	18	0·5	18	4·5		
	19	4·9	19	7·3	19	10·9	19	13·2	19	16·8	19	19·2	18	23·1	19	3·1		
	20	3·6	20	5·9	20	9·6	20	11·8	20	15·4	20	17·8	19	21·8	20	1·7		
	21	2·2	21	4·5	21	8·2	21	10·4	21	14·1	21	16·4	20	20·4	21	0·3		
	22	0·8	22	3·2	22	6·8	22	9·1	22	12·7	22	15·1	21	19·0	21	22·9		
	22	23·4	23	1·8	23	5·4	23	7·7	23	11·3	23	13·7	22	17·6	22	21·6		
	23	22·0	24	0·4	24	4·0	24	6·3	24	9·9	24	12·3	23	16·2	23	20·2		
	24	20·7	24	23·0	25	2·7	25	4·9	25	8·6	25	10·9	24	14·9	24	18·8		
	25	19·3	25	21·6	26	1·3	26	3·5	26	7·2	26	9·5	25	13·5	25	17·4		
	26	17·9	26	20·3	26	23·9	27	2·1	27	5·8	27	8·2	26	12·1	26	16·0		
	27	16·5	27	18·9	27	22·5	28	0·7	28	4·4	28	6·8	27	10·7	27	14·7		
	28	15·1	28	17·5	28	21·1	28	23·3	29	3·0	29	5·4	28	9·3	28	13·3		
	29	13·7	29	16·1	29	19·7	29	21·9	30	1·7	30	4·0	29	8·0	29	11·9		
	30	12·3	30	14·7	30	18·3	30	20·5	31	0·3	31	2·6	30	6·6	30	10·5		
	31	10·9			31	16·9			31	22·9					31	9·2		

ENCELADUS.

h.	March.		April.		May.		June.		July.		Aug.		Sept.		Oct.		Nov.	
h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
22·7	2	0·5	1	4·1	1	7·4	1	19·6	1	22·9	1	2·2	1	14·5	1	18·2	2	6·8
7·6	3	9·4	2	13·0	2	16·3	3	4·5	3	7·7	2	11·1	2	23·4	3	3·1	3	15·7
16·5	4	18·3	3	21·8	4	1·2	4	13·3	4	16·6	3	20·0	4	8·3	4	12·0	5	0·6
1·4	6	3·2	5	6·7	5	10·1	5	22·2	6	1·5	5	4·8	5	17·2	5	20·9	6	9·5
10·3	7	12·0	6	15·6	6	19·0	7	7·1	7	10·4	6	13·7	7	2·1	7	5·8	7	18·4
19·1	8	20·9	8	0·5	8	3·8	8	16·0	8	19·2	7	22·6	8	11·0	8	14·7		
4·0	10	5·8	9	9·4	9	12·7	10	0·8	10	4·1	9	7·5	9	19·9	9	23·6		
12·9	11	14·7	10	18 2	10	21·6	11	9·7	11	13·0	10	16·4	11	4·8	11	8·5		
21·8	12	23·6	12	3·1	12	6·5	12	18·6	12	21·9	12	1·2	12	13·7	12	17·3		
6·7	14	8·5	13	12·0	13	15·4	14	3·5	14	6·8	13	10·1	13	22·6	14	2·2		
15·6	15	17·4	14	20·9	15	0·2	15	12·4	15	15·6	14	19·0	15	7·4	15	11·1		
	17	2·3	16	5·8	16	9·1	16	21·3	17	0·5	16	3·9	16	16·3	16	20·0		
	18	11·2	17	14·6	17	18·0	18	6·1	18	9·4	17	12·8	18	1·2	18	4·9		
	19	20·0	18	23·5	19	2·9	19	15·0	19	18·3	18	21·7	19	10·1	19	13·8		
	21	4·9	20	8·4	20	11·8	20	23·9	21	3·2	20	6·6	20	19·0	20	22·7		
	22	13·8	21	17·3	21	20·6	22	8·8	22	12·0	21	15·5	22	3·9	22	7·6		
	23	22·7	23	2·2	23	5·5	23	17·7	23	20·9	23	0·4	23	12·8	23	16·5		
	25	7·6	24	11·0	24	14·4	25	2·5	25	5·8	24	9·3	24	21·7	25	1·4		
	26	16·5	25	19·9	25	23·3	26	11·4	26	14·7	25	18·1	26	6·6	26	10·3		
	28	1·4	27	4·8	27	8·2	27	20·3	27	23·6	27	3·0	27	15·5	27	19·2		
	29	10·3	28	13·7	28	17·0	29	5·2	29	8·4	28	11·9	29	0·4	29	4·1		
	30	19·2	29	22·6	30	1·9	30	14·0	30	17·3	29	20·8	30	9·3	30	13·0		
					31	10·7					31	5·7			31	21·9		

East Elongations.

TETHYS.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	No
d h	d h	d h	d h	d h	d h	d h	d h	d h	d
14 9·1	1 11·7	2 14·0	2 18·9	1 23·6	2 4·2	1 8·9	2 11·0	2 16·2	1 2
16 6·4	3 9·0	4 11·3	4 16·2	3 20·9	4 1·5	3 6·2	4 8·4	4 13·5	
18 3·7	5 6·3	6 8·6	6 13·5	5 18·2	5 22·8	5 3·5	6 5·7	6 10·8	
20 1·1	7 3·7	8 6·0	8 10·8	7 15·4	7 20·0	7 0·8	8 3·0	8 8·1	
21 22·4	9 1·0	10 3·3	10 8·1	9 12·7	9 17·3	8 22·1	10 0·3	10 5·4	
23 19·7	10 22·3	12 0·6	12 5·4	11 10·0	11 14·6	10 19·4	11 21·6	12 2·8	
25 17·0	12 19·6	13 21·9	14 2·7	13 7·3	13 11·9	12 16·7	13 19·0	14 0·1	
27 14·3	14 16·9	15 19·2	15 23·9	15 4·6	15 9·2	14 14·0	15 16·3	15 21·4	
	16 14·2	17 16·5	17 21·2	17 1·9	17 6·5	16 11·3	17 13·6	17 18·7	
	18 11·5	19 13·8	19 18·5	18 23·2	19 3·8	18 8·6	19 10·9	19 16·1	
	20 8·8	21 11·1	21 15·8	20 20·5	21 1·1	20 5·9	21 8·2	21 13·4	
	22 6·1	23 8·4	23 13·1	22 17·7	22 22·4	22 3·2	23 5·5	23 10·8	
	24 3·5	25 5·7	25 10·4	24 15·0	24 19·7	24 0·5	25 2·8	25 8·1	
	26 0·8	27 3·0	27 7·7	26 12·3	26 17·0	25 21·8	27 0·2	27 5·5	
	27 22·1	29 0·3	29 5·0	28 9·6	28 14·3	27 19·1	28 21·5	29 2·8	
	29 19·4	30 21·6	31 2·3	30 6·9	30 11·6	29 16·4	30 18·8	31 0·1	
	31 16·7					31 13·7			

DIONE.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	No
d h	d h	d h	d h	d h	d h	d h	d h	d h	d
14 3·7	2 14·0	1 16·8	1 19·2	3 15·1	3 17·3	2 19·4	1 21·9	2 0·7	1
16 21·4	5 7·7	4 10·5	4 12·9	6 8·8	6 10·9	5 13·1	4 15·6	4 18·4	3 2
19 15·1	8 1·4	7 4·2	7 6·6	9 2·4	9 4·6	8 6·8	7 9·3	7 12·1	6 1
22 8·9	10 19·2	9 21·8	10 0·3	11 20·1	11 22·2	11 0·5	10 3·0	10 5·9	
25 2·6	13 12·9	12 15·5	12 17·9	14 13·7	14 15·9	13 18·1	12 20·7	12 23·6	
27 20·3	16 6·6	15 9·2	15 11·6	17 7·4	17 9·5	16 11·8	15 14·4	15 17·3	
	19 0·3	18 2·9	18 5·2	20 1·0	20 3·2	19 5·5	18 8·1	18 11·0	
	21 18·0	20 20·6	20 22·9	22 18·7	22 20·8	21 23·2	21 1·8	21 4·8	
	24 11·7	23 14·3	23 16·5	25 12·3	25 14·5	24 16·8	23 19·6	23 22·5	
	27 5·4	26 7·9	26 10·2	28 6·0	28 8·1	27 10·5	26 13·3	26 16·3	
	29 23·1	29 1·6	29 3·8	30 23·6	31 1·8	30 4·2	29 7·0	29 10·0	
			13 21·5						

RHEA.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	No
d h	d h	d h	d h	d h	d h	d h	d h	d h	d
13 0·0	3 2·1	3 17·4	5 8·2	1 10·3	3 0·4	3 14·8	4 5·6	1 8·5	2
17 12·5	7 14·6	8 5·9	9 20·6	5 22·6	7 12·7	8 3·2	8 18·1	5 21·0	6 1
22 1·1	12 3·1	12 18·3	14 8·9	10 10·9	12 1·1	12 15·6	13 6·5	10 9·5	11
26 13·6	16 15·6	17 6·7	18 21·3	14 23·2	16 13·4	17 4·0	17 19·0	14 22·0	
	21 4·0	21 19·1	23 9·6	19 11·5	21 1·8	21 16·4	22 7·5	19 10·5	
	25 16·5	26 7·5	27 21·9	23 23·8	25 14·1	26 4·8	26 20·0	23 23·1	
	30 5·0	30 19·9		28 12·1	30 2·5	30 17·2		28 11·6	

TITAN.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	No
d h	d h	d h	d h	d h	d h	d h	d h	d h	d
20 22·3	8 22·1	9 20·8	11 17·5	12 12·7	14 7·4	15 3·1	16 0·5	2 0·0	3
	24 21·7	25 19·1	27 15·2	28 9·9	30 5·0	31 1·6		17 23·8	

East Elongations.

HYPERION.

Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.
h	d h	d h	d h	d h	d h	d h	d h	d h	d h
12·5	15 22·8	6 8·6 27 17·5	19 1·4	9 8·9 30 15·8	21 22·8	12 6·0	2 14·2 23 22·3	15 7·2	5 16·8

IAPETUS.

	d h	d h	d h	d h
E. Elongation	Mar. 6 18·7,	May 25 1·0,	Aug. 11 2·9,	Oct. 29 23·5
Inf. Conjunction ...	Mar. 26 3·1,	June 12 21·8,	Aug. 30 5·5,	Nov. 18 16·1
W. Elongation	Apr. 15 19·9,	July 3 3·8,	Sept. 19 21·8,	
Sup. Conjunction ...	Feb. 15 8·4,	May 6 5·3,	July 23 6·5,	Oct. 10 13·9

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 10	6 6·2	35·15	15·76	26 38·9 N.	26 44·1 N.
30	6 16·9	35·69	15·88	26 25·5 N.	26 42·5 N.
Feb. 19	6 25·3	36·53	16·13	26 11·9 N.	26 40·7 N.
Mar. 11	6 31·2	37·62	16·49	26 0·3 N.	26 38·8 N.
31	6 34·4	38·88	16·97	25 53·0 N.	26 36·8 N.
April 20	6 34·8	40·18	17·52	25 51·4 N.	26 34·5 N.
May 10	6 32·6	41·35	18·08	25 55·7 N.	26 31·9 N.
30	6 28·1	42·21	18·56	26 5·1 N.	26 29·3 N.
June 19	6 21·9	42·61	18·87	26 17·0 N.	26 26·4 N.
July 9	6 15·1	42·45	18·93	26 29·3 N.	26 23·3 N.
29	6 9·1	41·78	18·75	26 39·8 N.	26 20·1 N.
Aug. 18	6 5·3	40·72	18·35	26 47·3 N.	26 16·7 N.
Sept. 7	6 4·6	39·46	17·83	26 51·4 N.	26 13·0 N.
27	6 7·2	38·18	17·25	26 51·8 N.	26 9·2 N.
Oct. 17	6 12·9	37·01	16·68	26 47·9 N.	26 5·3 N.
Nov. 6	6 20·8	36·05	16·17	26 39·2 N.	26 1·1 N.
26	6 30·0	35·36	15·74	26 25·7 N.	25 56·8 N.
Dec. 16	6 39·5	34·99	15·40	26 7·1 N.	25 52·2 N.

SATELLITES OF URANUS.

North Elongations.

ARIEL.

April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h
1 20·0	2 2·7	1 9·4	1 16·2	3 11·1	2 17·2
4 8·5	4 15·2	3 21·9	4 4·7	5 23·6	5 5·7
6 21·1	7 3·8	6 10·5	6 17·3	8 12·2	7 18·2
9 9·7	9 16·4	8 23·1	9 5·8	11 0·7	10 6·7
11 22·2	12 4·9	11 11·6	11 18·3	13 13·3	12 19·2
14 10·7	14 17·5	14 0·2	14 6·9	16 1·8	15 7·7
16 23·3	17 6·1	16 12·8	16 19·4	18 14·3	17 20·2
19 11·8	19 18·6	19 1·3	19 7·9	21 2·8	20 8·7
22 0·4	22 7·2	21 13·9	21 20·5	23 15·3	22 21·2
24 13·0	24 19·8	24 2·5	24 9·0	26 3·8	25 9·7
27 1·5	27 8·3	26 15·0	26 21·5	28 16·2	27 22·2
29 14·1	29 20·9	29 3·6	29 10·0	31 4·7	30 10·7
			31 22·6		

UMBRIEL.

March.	April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h	d h
	1 2·9	4 8·0	2 9·6	1 11·2	3 16·0	1 16·3
	5 6·5	8 11·6	6 13·2	5 14·9	7 19·6	5 19·8
	9 10·1	12 15·2	10 16·9	9 18·5	11 23·1	9 23·2
	13 13·8	16 18·9	14 20·5	13 22·2	16 2·6	14 2·6
	17 17·4	20 22·5	19 0·2	18 1·8	20 6·1	18 6·0
19 15·9	21 21·1	25 2·2	23 3·8	22 5·4	24 9·5	22 9·4
23 19·5	26 0·7	29 5·9	27 7·6	26 8·9	28 13·0	26 12·9
27 23·2	30 4·4			30 12·5		30 16·3
						October.
						4 19·8

TITANIA.

March.	April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h	d h
8 12·6	3 17·4	8 16·5	3 21·9	8 20·9	4 1·4	7 21·3
17 6·1	12 11·2	17 10·3	12 15·8	17 14·5	12 18·6	16 14·0
25 23·7	21 4·9	26 4·1	21 9·6	26 8·0	21 11·6	25 6·6
	29 22·7		30 3·3		30 4·5	October.
						3 23·1

OBERON.

April.	May.	June.	July.	August.	September.
d h	d h	d h	d h	d h	d h
5 1·3	2 3·5	11 19·4	8 21·3	4 22·0	14 6·4
18 14·4	15 16·8	25 8·5	22 9·9	18 9·4	
	29 6·1			31 20·2	

Apparent Apsides.

		Apparent Distances.			
	Position Angle.	Ariel.	Umbriel.	Titania.	Oberon.
	°	"	"	"	"
April 1	302·9	14·3	19·9	32·7	43·7
June 1	313·7	14·7	20·5	33·6	44·9
August 1	323·3	14·3	19·9	32·7	43·7

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.	February.	March.	September.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
5 20·8	4 6·3	5 15·7	3 17·9	3 3·1	1 12·4	6 19·0
11 17·9	10 3·4	11 12·8	9 14·9	9 0·1	7 9·5	12 16·1
17 15·0	16 0·5		15 12·0	14 21·2	13 6·6	18 13·2
23 12·1	21 21·6		21 9·0	20 18·2	19 3·7	24 10·3
29 9·2	27 18·7		27 6·0	26 15·3	25 0·8	30 7·4
					30 21·9	

Position [illegible] Jan. 16, 260°·1, 16h·4; Sept. 18, 265°·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1900.	P.	D.	L.	1900.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 1 54	−3 15	34 4	July 5	− 0 47	+3 30	112 13
6	− 0 33	3 48	328 13	10	+ 1 29	4 1	46 3
11	2 58	4 20	262 22	15	3 45	4 30	339 53
16	5 21	4 50	196 31	20	5 57	4 58	273 44
21	7 39	5 18	130 41	25	8 6	5 23	207 35
26	9 53	5 43	64 51	30	10 11	5 46	141 27
31	12 1	6 5	359 1	Aug. 4	12 11	6 7	75 20
Feb. 5	14 1	6 25	293 11	9	14 4	6 25	9 13
10	15 55	6 41	227 21	14	15 52	6 41	303 6
15	17 40	6 55	161 30	19	17 32	6 54	237 2
20	19 16	7 5	95 40	24	19 6	7 4	170 58
25	20 44	7 11	29 48	29	20 31	7 10	104 54
Mar. 2	22 3	7 15	323 57	Sept. 3	21 48	7 14	38 51
7	23 12	7 15	258 4	8	22 57	7 15	332 49
12	24 11	7 11	192 12	13	23 57	7 12	266 48
17	24 59	7 5	126 17	18	24 47	7 7	200 48
22	25 38	6 55	60 22	23	25 28	6 58	134 48
27	26 6	6 42	354 25	28	25 59	6 46	68 49
Apr. 1	26 23	6 26	288 28	Oct. 3	26 19	6 31	2 51
6	26 30	6 8	222 29	8	26 29	6 14	296 53
11	26 26	5 46	156 30	13	26 28	5 53	230 55
16	26 10	5 23	90 28	18	26 16	5 30	164 58
21	25 44	4 57	24 26	23	25 52	5 4	99 1
26	25 6	4 28	318 22	28	25 16	4 36	33 5
May 1	24 17	3 59	252 18	Nov. 2	24 29	4 5	327 9
6	23 18	3 27	186 12	7	23 29	3 33	261 13
11	22 8	2 54	120 4	12	22 18	2 59	195 18
16	20 48	2 20	53 56	17	20 55	2 23	129 24
21	19 18	1 45	347 48	22	19 22	1 47	63 29
26	17 39	1 10	281 38	27	17 37	1 9	357 35
31	15 51	−0 34	215 29	Dec. 2	15 43	+0 31	291 42
June 5	13 56	+0 3	149 19	7	13 40	−0 7	225 49
10	11 54	0 39	83 8	12	11 30	0 46	159 56
15	9 47	1 14	16 56	17	9 13	1 24	94 4
20	7 36	1 50	310 45	22	6 52	2 1	28 13
25	5 21	2 24	234 34	27	4 27	2 38	322 21
30	− 3 5	+2 58	178 24	32	+ 2 1	−3 13	256 29

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 23′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1900·0.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	V Sculptoris	0 3 34	−39 47·1	56.	S Camelopardi	5 30 13	+68 44·6
2.	S Sculptoris	0 10 19	−32 36·1	57.	U Aurigæ	5 35 38	+31 59·4
3.	T Andromedæ	0 17 10	+26 26·4	58.	R Columbæ	5 46 40	−29 13·2
4.	T Cassiopeiæ	0 17 49	+55 14·3	59.	U Orionis	5 49 53	+20 9·4
5.	S Toucani	0 18 24	−62 14·0	60.	η Geminorum	6 8 50	+22 32·1
6.	R Andromedæ	0 18 45	+38 1·4	61.	V Aurigæ	6 16 18	+47 42·7
7.	S Ceti	0 18 58	− 9 52·9	62.	V Monocerotis	6 17 41	− 2 8·7
8.	T Sculptoris	0 24 17	−38 27·7	63.	T Monocerotis	6 19 49	+ 7 8·4
9.	U Cassiopeiæ	0 40 46	+47 42·6	64.	W Geminorum	6 29 14	+15 24·5
10.	V Andromedæ	0 44 39	+35 6·5	65.	S Lyncis	6 35 56	+58 0·5
11.	X Sculptoris	0 44 42	−35 27·8	66.	X Geminorum	6 40 43	+30 22·6
12.	W Cassiopeiæ	0 48 58	+58 1·3	67.	S Monocerotis	6 35 28	+ 9 59·2
13.	U Cephei	0 53 24	+81 20·2	68.	W Monocerotis	6 47 30	− 7 1·6
14.	U Sculptoris	1 6 50	−30 38·8	69.	R Lyncis	6 53 3	+55 28·3
15.	U Andromedæ	1 9 47	+40 11·4	70.	ζ Geminorum	6 58 11	+20 43·0
16.	S Cassiopeiæ	1 12 18	+72 5·1	71.	R Geminorum	7 1 20	+22 51·5
17.	S Piscium	1 12 21	+ 8 24·3	72.	V Canis Min.	7 1 33	+ 9 1·5
18.	U Piscium	1 17 41	+12 20·6	73.	R Canis Min.	7 3 13	+10 10·9
19.	R Sculptoris	1 22 22	−33 3·7	74.	L² Puppis	7 10 29	−44 28·8
20.	R Piscium	1 25 29	+ 2 22·0	75.	R Canis Maj.	7 14 56	−16 12·4
21.	X Cassiopeiæ	1 49 45	+58 46·1	76.	V Geminorum	7 17 34	+13 17·0
22.	U Persei	1 52 56	+54 20·2	77.	U Monocerotis	7 26 2	− 9 34·0
23.	S Arietis	1 59 16	+12 2·8	78.	S Canis Min.	7 27 18	+ 8 31·9
24.	R Arietis	2 10 26	+24 35·5	79.	T Canis Min.	7 28 26	+11 57·5
25.	o Ceti	2 14 18	− 3 25·9	80.	X Puppis	7 28 26	−20 41·7
26.	R Ceti	2 20 55	− 0 37·8	81.	U Canis Min.	7 35 55	+ 8 36·8
27.	S Horologii	2 22 22	−60 1·2	82.	S Geminorum	7 37 3	+23 41·2
28.	R Fornacis	2 24 47	−26 32·5	83.	W Puppis	7 42 39	−41 57·1
29.	U Ceti	2 28 56	−13 35·2	84.	T Geminorum	7 43 18	+23 59·0
30.	R Trianguli	2 30 59	+33 49·7	85.	U Geminorum	7 49 10	+22 15·9
31.	T Arietis	2 42 45	+17 5·6	86.	V Puppis	7 55 22	−48 58·4
32.	V Persei	2 43 15	+56 34·1	87.	U Puppis	7 56 8	−12 33·8
33.	R Horologii	2 49 42	−50 21·3	88.	Y Puppis	8 8 50	−34 50·3
34.	S Horologii	2 57 40	−51 2·2	89.	R Cancri	8 11 3	+12 2·0
35.	Algol	3 1 39	+40 34·2	90.	V Cancri	8 16 1	+17 36·2
36.	U Arietis	3 5 30	+14 25·3	91.	V Carinæ	8 26 41	−59 47·1
37.	X Ceti	3 14 20	− 1 26·0	92.	X Carinæ	8 29 7	−58 53·2
38.	R Persei	3 23 41	+35 19·6	93.	U Cancri	8 30 3	+19 14·4
39.	U Eridani	3 46 14	−25 15·6	94.	T Velorum	8 34 26	−47 0·7
40.	T Eridani	3 50 56	−24 19·5	95.	S Cancri	8 38 14	+19 23·6
41.	λ Tauri	3 55 8	+12 12·5	96.	R Mali	8 41 17	−27 50·2
42.	W Tauri	4 22 18	+15 53·1	97.	S Hydræ	8 48 21	+ 3 26·8
43.	R Tauri	4 22 49	+ 9 56·4	98.	T Hydræ	8 50 48	− 8 45·6
44.	S Tauri	4 23 43	+ 9 43·5	99.	T Cancri	8 50 58	+20 13·9
45.	T Camelopardi	4 30 21	+65 56·7	100.	S Mali	9 0 40	−24 41·4
46.	R Reticuli	4 32 30	−63 14·2	101.	W Cancri	9 4 2	+25 39·4
47.	V Tauri	4 46 14	+17 22·2	102.	W Carinæ	9 19 15	−55 32·0
48.	R Orionis	4 53 35	+ 7 58·8	103.	S Antliæ	9 27 56	−28 11·2
49.	R Leporis	4 55 3	−14 57·4	104.	N Velorum	9 28 11	−56 35·6
50.	T Leporis	5 0 35	−22 2·4	105.	S Velorum	9 29 27	−44 45·9
51.	V Orionis	5 0 47	+ 3 57·9	106.	U Velorum	9 29 28	−45 4·3
52.	S Pictoris	5 8 18	−48 37·7	107.	R Carinæ	9 29 44	−62 20·8
53.	R Aurigæ	5 9 13	+53 28·5	108.	X Hydræ	9 30 44	−14 14·7
54.	T Columbæ	5 15 38	−33 48·7	109.	R Leo. Min.	9 39 36	+34 58·4
55.	*S Orionis*	5 24 4	− 4 46·3	110.	Z Hydræ	9 40 24	−23 13·5

Mean Places of Variable Stars for 1900·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
111.	R Leonis	9 42 11	+11 53·6	166.	T Triang. Austr.	15 0 24	−68 20·1
112.	*l* Carinæ	9 42 30	−62 2·8	167.	T^2 Libræ	15 0 47	−18 20·8
113.	V Leonis	9 54 28	+21 44·4	168.	T Libræ	15 5 2	−19 38·3
114.	S Carinæ	10 6 11	−61 3·3	169.	Y Libræ	15 6 24	− 5 38·0
115.	Y Carinæ	10 29 25	−57 58·6	170.	R Triang. Austr.	15 10 49	−66 7·7
116.	R Ursæ Maj.	10 37 35	+69 18·0	171.	U Coronæ	15 14 7	+32 0·8
117.	R^2 Hydræ	10 46 33	−28 6·1	172.	S Libræ	15 15 38	−20 1·6
118.	V Hydræ	10 46 46	−20 43·1	173.	S Serpentis	15 16 59	+14 40·4
119.	W Leonis	10 48 21	+14 14·9	174.	S Coronæ	15 17 19	+31 43·6
120.	U Carinæ	10 53 44	−59 11·8	175.	S^2 Libræ	15 18 29	−22 33·2
121.	S Leonis	11 5 41	+ 6 0·2	176.	U^2 Libræ	15 27 41	−14 59·3
122.	S^2 Centauri	11 16 6	−61 19·6	177.	X Libræ	15 30 26	−20 49·9
123.	X Centauri	11 44 12	−41 11·9	178.	W Libræ	15 32 12	−15 50·6
124.	W Centauri	11 50 2	−58 41·8	179.	S Ursæ Majoris	15 33 27	+78 58·3
125.	R Comæ	11 59 8	+19 20·5	180.	U Libræ	15 36 13	−20 51·5
126.	S Muscæ	12 7 24	−69 35·7	181.	Z Libræ	15 40 41	−20 48·6
127.	T Virginis	12 9 29	− 5 28·8	182.	V Coronæ	15 45 57	+39 52·3
128.	R Corvi	12 14 27	−18 41·6	183.	R Serpentis	15 46 5	+15 26·4
129.	T Crucis	12 15 54	−61 43·6	184.	R Libræ	15 47 56	−15 56·3
130.	R Crucis	12 18 8	−61 4·5	185.	R^2 Libræ	15 50 39	−18 0·6
131.	T Canum Ven.	12 25 15	+32 3·4	186.	S Triang. Austr.	15 52 12	−63 29·5
132.	Y Virginis	12 28 43	− 3 52·2	187.	U Triang. Austr.	15 58 25	−62 38·3
133.	T Ursæ Maj.	12 31 50	+60 2·3	188.	Z^2 Scorpii	15 58 37	−23 49·5
134.	R Virginis	12 33 26	+ 7 32·3	189.	X Herculis	15 59 39	+47 30·7
135.	R Muscæ	12 35 58	−68 51·5	190.	Z Scorpii	16 0 8	−21 27·6
136.	S Ursæ Maj.	12 39 34	+61 38·4	191.	R Herculis	16 1 43	+18 38·4
137.	U^2 Virginis	12 42 13	+ 4 41·5	192.	X Scorpii	16 2 40	−21 15·4
138.	U Virginis	12 46 1	+ 6 5·9	193.	W Scorpii	16 5 55	−19 52·6
139.	S Crucis	12 48 27	−57 53·3	194.	U^2 Herculis	16 6 3	+25 19·9
140.	W Virginis	13 20 52	− 2 51·6	195.	S Normæ	16 10 34	−57 39·2
141.	V Virginis	13 22 38	− 2 39·3	196.	R Scorpii	16 11 41	−22 41·9
142.	R Hydræ	13 24 15	−22 45·8	197.	S Scorpii	16 11 43	−22 38·8
143.	S Virginis	13 27 47	− 6 40·9	198.	W Ophiuchi	16 16 1	− 7 27·6
144.	Z Centauri	13 34 16	−31 7·5	199.	V Ophiuchi	16 21 10	−12 12·0
145.	T Centauri	13 36 2	−33 5·5	200.	U Herculis	16 21 23	+19 6·9
146.	T^2 Centauri	13 42 30	−36 21·8	201.	Y Scorpii	16 23 49	−19 13·3
147.	W Hydræ	13 43 23	−27 52·0	202.	T Ophiuchi	16 28 1	−15 55·1
148.	R Canum Ven.	13 44 39	+40 2·4	203.	S Ophiuchi	16 28 30	−16 57·0
149.	R^2 Virginis	13 59 35	− 8 43·1	204.	R Aræ	16 31 26	−56 47·6
150.	Z Virginis	14 4 58	−12 49·8	205.	W Herculis	16 31 41	+37 32·6
151.	S^2 Hydræ	14 5 48	−28 24·8	206.	Y Herculis	16 32 0	+ 7 18·6
152.	R Centauri	14 9 22	−59 26·8	207.	R Draconis	16 32 24	+66 57·8
153.	R^2 Centauri	14 9 55	−57 23·3	208.	S Herculis	16 47 20	+15 6·6
154.	Y Boötis	14 17 22	+20 15·8	209.	S^2 Scorpii	16 48 22	−44 56·3
155.	X Boötis	14 19 27	+16 46·5	210.	R^2 Scorpii	16 50 15	−30 25·3
156.	S Boötis	14 19 32	+54 15·9	211.	V^2 Scorpii	16 51 47	−33 27·2
157.	S^2 Virginis	14 22 16	+ 5 7·6	212.	V^2 Herculis	16 56 45	+31 22·3
158.	R Camelopardi	14 25 6	+84 17·2	213.	R Ophiuchi	17 2 1	−15 57·6
159.	V Centauri	14 25 23	−56 26·7	214.	T^2 Herculis	17 6 47	+27 10·8
160.	V Boötis	14 25 43	+39 18·4	215.	W^2 Scorpii	17 8 18	−33 19·0
161.	R Boötis	14 32 47	+27 10·2	216.	U Ophiuchi	17 11 27	+ 1 19·3
162.	V Libræ	14 34 48	−17 13·6	217.	Z Ophiuchi	17 14 28	+ 1 37·3
163.	S Lupi	14 46 42	−46 12·0	218.	S^2 Herculis	17 17 31	+23 1·1
164.	U Boötis	14 49 42	+18 6·1	219.	U^2 Scorpii	17 35 6	−43 42·3
165.	δ Libræ	14 55 38	− 8 7·3	220.	X Sagittarii	17 41 16	−27 47·5

Mean Places of Variable Stars for 1900·0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
221.	Y^2 Scorpii	17 44 16	−33 40·5	278.	U Cygni	20 16 30	+47 34·7
222.	Y Ophiuchi	17 47 17	− 6 7·1	279.	S^3 Cygni	20 29 55	+54 37·6
223.	Z Herculis	17 53 36	+15 8·4	280.	W Delphini	20 33 7	+17 55·9
224.	T Draconis	17 54 52	+58 13·7	281.	R Microscopii	20 33 58	−29 8·6
225.	W Sagittarii	17 58 39	−29 35·2	282.	V Cygni	20 38 5	+47 47·0
226	T Herculis	18 5 19	+31 0·1	283.	S Delphini	20 38 28	+16 43·7
227.	S^2 Sagittarii	18 10 58	−34 8·5	284.	Y Aquarii	20 39 9	− 5 12·0
228.	W Lyræ	18 11 28	+36 38·3	285.	X Cygni	20 39 29	+35 13·5
229.	Y Sagittarii	18 15 30	−18 54·3	286.	T Delphini	20 40 43	+16 2·1
230.	V^2 Sagittarii	18 21 21	−33 22·7	287.	W Aquarii	20 41 10	− 4 26·9
231.	*d* Serpentis	18 22 5	+ 0 8·2	288.	V Aquarii	20 41 46	+ 2 4·3
232.	T Serpentis	18 23 56	+ 6 13·8	289.	U Capricorni	20 42 34	−15 9·1
233.	U Sagittarii	18 26 0	−19 11·9	290.	R^2 Cygni	20 42 37	+44 30·2
234.	X Ophiuchi	18 33 35	+ 8 44·4	291.	V Delphini	20 43 14	+18 58·0
235.	U Coron. Austr.	18 34 17	−37 55·6	292.	T Aquarii	20 44 40	− 5 31·1
236.	R Scuti	18 42 9	− 5 48·8	293.	U Pavonis	20 47 10	−63 5·2
237.	β Lyræ	18 46 23	+33 14·8	294.	T Vulpeculæ	20 47 14	+27 52·3
238.	κ Pavonis	18 46 38	−67 21·5	295.	Y Cygni	20 48 5	+34 16·9
239.	R Lyræ	18 52 17	+43 48·8	296.	Z^2 Cygni	20 48 32	+46 58·7
240.	R Coron. Austr.	18 55 9	−37 5·7	297.	X Delphini	20 50 18	+17 15·7
241.	R Aquilæ	19 1 33	+ 8 4·7	298.	R^2 Capricorni	20 56 23	−27 29·0
242.	V Lyræ	19 5 9	−29 29·9	299.	R Vulpeculæ	20 59 56	+23 25·5
243.	X^2 Sagittarii	19 8 42	−18 58·8	300.	V Capricorni	21 1 48	−24 19·7
244.	S Lyræ	19 9 6	+25 50·0	301.	X Capricorni	21 2 50	−21 45·0
245.	U Draconis	19 9 57	+67 6·9	302.	Z Capricorni	21 5 3	−16 35·2
246.	W Aquilæ	19 10 0	− 7 13·0	303.	T Cephei	21 8 13	+68 5·0
247.	T Sagittarii	19 10 28	−17 8·8	304.	T Capricorni	21 16 30	−15 34·9
248.	R Sagittarii	19 10 28	−19 29·0	305.	S Microscopii	21 20 48	−30 17·0
249.	S Sagittarii	19 13 35	−19 12·4	306.	Y Capricorni	21 28 55	−14 25·1
250.	Z Sagittarii	19 13 47	−21 6·6	307.	W Cygni	21 32 16	+44 55·7
251.	U Lyræ	19 16 37	+37 41·6	308.	S Cephei	21 36 28	+78 10·5
252.	T Sagittæ	19 17 14	+17 27·9	309.	U^2 Cygni	21 37 16	+53 52·3
253.	U Aquilæ	19 23 58	− 7 15·0	310.	R^3 Cygni	21 38 46	+43 7·6
254.	U Vulpeculæ	19 32 16	+20 6·6	311.	R Gruis	21 42 6	−47 22·0
255.	T^2 Aquilæ	19 33 17	+11 28·9	312.	V Pegasi	21 56 2	+ 5 38·4
256.	R Cygni	19 34 8	+49 58·6	313.	U Aquarii	21 57 52	−17 6·5
257.	T Pavonis	19 39 31	−72 0·7	314.	S Piscis Austr.	21 58 2	−28 32·0
258.	T^3 Cygni	19 40 48	+29 0·4	315.	T Pegasi	22 4 1	+12 3·0
259.	T^2 Cygni	19 40 50	+48 31·9	316.	R Piscis Austr.	22 12 19	−30 6·2
260.	S Vulpeculæ	19 44 18	+27 2·2	317.	X Aquarii	22 13 9	−21 24·0
261.	X Aquilæ	19 46 31	+ 4 12·5	318.	S Gruis	22 19 55	−48 56·8
262.	χ Cygni	19 46 44	+32 39·7	319.	S Lacertæ	22 24 38	+39 48·2
263.	η Aquilæ	19 47 23	+ 0 44·9	320.	δ Cephei	22 25 27	+57 54·2
264.	R^2 Sagittarii	19 49 43	−29 27·0	321.	W Cephei	22 32 39	+57 54·6
265.	S Sagittæ	19 51 28	+16 22·1	322.	R Lacertæ	22 38 50	+41 50·9
266.	R^2 Aquilæ	19 52 25	− 2 9·1	323.	S Aquarii	22 51 45	−20 52·6
267.	S^2 Aquilæ	19 53 43	− 8 9·3	324.	R Pegasi	23 1 38	+10 0·2
268.	Z Cygni	19 58 37	+49 45·8	325.	V Cassiopeiæ	23 7 22	+59 8·4
269.	S Cygni	20 3 24	+57 41·9	326.	W Pegasi	23 14 46	+25 43·5
270.	U^3 Cygni	20 3 50	+46 0·6	327.	S Pegasi	23 15 29	+ 8 22·3
271.	R Capricorni	20 5 42	−14 34·0	328.	R Aquarii	23 38 39	−15 50·3
272.	S Aquilæ	20 7 1	+15 19·4	329.	V Cephei	23 51 44	+82 38·1
273.	W Capricorni	20 8 3	−22 17·0	330.	V Ceti	23 52 47	− 9 31·0
274.	R Sagittæ	20 9 30	+16 25·3	331.	U Pegasi	23 52 52	+15 23·9
275.	Z Aquilæ	20 9 51	− 6 27·4	332.	R Cassiopeiæ	23 53 19	+50 49·9
276.	*R Delphini*	20 10 5	+ 8 47·1	333.	W Ceti	23 57 0	−15 13·9
277.	*T^2 Sagittarii*	20 11 5	−39 25·2				

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's name refers to Catalogue on pp. 24–26.)

Note.—The times of minima only are given; the times of maxima may be found by adding the inte
printed under the name of the star.

T Monocer. 63.

($7^d\ 22\frac{1}{2}^h$.)

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
d h 8 16	4 16	3 16 30 16	26 17	d h 23 17	19 17	16 18	12 18	d h 8 18	5 18	1 19 28 19	25 19

W Geminor. 64.

($2^d\ 15\frac{1}{2}^h$.)

Jan.	Feb.	Mar.	Apr.	May	Aug.	Sept.	Oct.	Nov.	Dec.
d h 2 4 9 22 17 16 25 10	2 3 9 21 17 15 25 9	5 2 12 20 20 14 28 8	5 2 12 19 d h 20 13 28 7	6 1 13 18	6 22 14 16 22 9 30 3	6 21 14 15 22 8 30 2	d h 7 20 15 14 23 7 31 1	7 19 15 13 23 7	1 0 8 18 16 12 24 6

ζ Geminor. 70.

($5^d\ 0^h$.)

Jan.	Feb.	Mar.	Apr.
d h 2 13 12 17 22 20	2 0 12 4 d h 22 8	4 11 14 15 24 19	d h 3 22 14 2 24 6

ζ Geminor. 70 (*continued*).

May	Aug.	Sept.	Oct.	Nov.	Dec.
d h 4 9 14 13 24 17	13 22 24 2	d h 3 6 13 9 23 13	3 17 13 20 24 0	d h 3 4 13 8 23 11	3 15 13 19 23 22

W Virginis 140.

($8^d\ 5^h$.)

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
d h 12 3 29 10	15 16	4 23 22 5	8 12 25 18	d h 13 1 30 7	16 14	3 20 21 3	7 9 24 16	d h 10 22 28 5	15 11	1 18 19 0	6 7 23 14

X Sagittar. 220.

($2^d\ 21^h$.)

Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
d h 1 14 8 15 15 15 22 15	1 16 8 16 15 16 22 16 29 17	5 17 12 17 19 18 26 18	3 18 10 18 d h 17 19 24 19 31 19	7 20 14 20 21 20 28 20	5 21 12 21 19 21 26 22	2 22 9 22 16 22 23 23 30 23	d h 6 23 13 0 20 0 28 0	5 0 12 1 19 1 26 1	2 1 9 2 16 2 23 2 30 3	7 3

Y Ophiuchi 222.

($6^d\ 5^h$.)

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.
d h 3 5 20 8	6 10 23 13	12 16 29 19	d h 15 22	3 1 20 4	6 7 23 10	10 13 27 15	d h 13 18 30 21	17 0	4 3 21 6

W Sagittar. 225.

($3^d\ 0^h$.)

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.
d h 31 11	8 2 15 16 23 6	2 21 10 11 18 1 25 15	2 6 9 20 17 10 25 0	d h 2 15 10 5 17 19 25 10	2 0 9 14 17 4 24 19	2 9 9 23 17 13 25 4	1 18 9 8 16 23 24 13	d h 1 3 8 17 16 8 23 22	1 12 9 2 16 17 24 7 31 21	8 11 16 2 23 16

Y Sagittar. 229.

($1^d\ 19^h$.)

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
d h 20 13 26 7	1 2 6 20 12 15 18 10 24 4	1 23 7 17 13 12 19 6 25 1 30 19	5 14 11 9 17 3 22 22 28 16	d h 4 11 10 5 16 0 21 19 27 13	2 8 8 2 13 21 19 15 25 10	1 4 6 23 12 18 18 12 24 7 30 1	4 20 10 14 16 9 22 3 29 22	d h 2 17 8 11 14 6 20 0 25 19	1 13 7 8 13 2 18 21 24 16 30 10	5 5 10 23 16 18 22 12 28 7	4 1

U Sagittar. 233.

($2^d\ 23\frac{1}{2}^h$.)

Jan.	Feb.	Mar.	Apr.	May
d h 13 22 20 15 27 9	3 3 9 21 16 15 23 9	d h 2 3 8 20 15 14 22 8 29 2	4 20 11 14 d h 18 8 25 2	1 19 8 13 15 7 22 1 28 19

U Sagittar (*continued*

June	July	Aug.	Sept.
d h 4 13 11 7 18 1 24 18	1 12 8 6 15 0 21 18 28 12	d h 4 6 10 23 17 17 24 11 31 5	6 23 13 17 20 11 27 5

β Lyræ 2

(Max. 3^d

Secondary M

$6^d\ 11^h$.

Max. 9^d 16

Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.
d h 8 13 21 11	3 9 16 7	1 5 14 3 27 1	8 23 21 21	d h 4 19 17 17 30 15	12 13 25 11	8 9 21 7	3 5 16 3 29 1

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 253.

(2^{d} 4^{h}.)

Month	d	h
Feb.	13	5
	20	5
	27	6
Mar.	6	7
	13	7
	20	8
	27	8
Apr.	3	9
	10	10
	17	10
	24	11
May	1	11
	8	12
	15	12
May	22	13
	29	14
June	5	14
	12	15
	19	15
	26	16
July	3	16
	10	17
	17	18
	24	18
	31	19
Aug.	7	19
	14	20
	21	20
	28	21
Sept.	4	22
	11	22
	18	23
	25	23
Oct.	3	0
	10	0
	17	1
	24	2
	31	2
Nov.	7	3
	14	3
	21	4
	28	5

η AQUILÆ 263.

(2^{d} 6^{h}.)

Month	d	h
Feb.	4	3
	11	7
	18	11
	25	16
Mar.	4	20
	12	0
	19	4
	26	8
Apr.	2	13
	9	17
	16	21
	24	1

η AQUILÆ 263 (*continued*).

Month	d	h
May	1	6
	8	10
	15	14
	22	18
	29	23
June	6	3
	13	7
	20	11
	27	15
July	4	20
	12	0
July	19	4
	26	8
Aug.	2	13
	9	17
	16	21
	24	1
	31	6
Sept.	7	10
	14	14
	21	18
	28	22
Oct.	6	3
	13	7
	20	11
	27	15
Nov.	3	20
	11	0
	18	4
	25	8
Dec.	2	13
	9	17
	16	21
	24	1
	31	6

S SAGITTÆ 265.

(3^{d} 10^{h}.)

Month	d	h
Jan.	21	14
	29	23
Feb.	7	9
	15	18
	24	3
Mar.	4	12
	12	21
	21	7
Apr.	7	1
	15	10
Apr.	23	19
May	2	4
	10	14
	18	23
	27	8

S SAGITTÆ 265 (*continued*).

Month	d	h
June	4	17
	13	2
	21	12
	29	21
July	8	6
	16	15
	25	0
Aug.	2	10
	10	19
	19	14
	27	13
Sept.	4	22
	13	8
	21	17
	30	2
Oct.	8	11
	16	20
	25	6
Nov.	2	15
	11	0
	19	9
	27	18
Dec.	6	4
	14	13

X CYGNI 285.

(6^{d} 19^{h}.)

Month	d	h
Jan.	14	17
	31	2
Feb.	16	12
Mar.	4	21
	21	6
Apr.	6	15
	23	1
May	9	10
	25	19
June	11	4
	27	14
July	13	23
	30	8
Aug.	15	17
Sept.	1	3
	17	12
Oct.	3	21
	20	6
Nov.	5	16
	22	1
Dec.	8	10
	24	19

T VULPEC. 294.

(1^{d} 9½^{h}.)

Month	d	h
Jan.	4	21
	9	7
	13	18
	18	4
	22	15
	27	1
	31	12
Feb.	4	22
	9	9
	13	19
	18	5
	22	16
	27	2
Mar.	3	13
	7	23
	12	10
	16	20
	21	7
	25	17
	30	4
Apr.	3	14
	8	1
	12	11
	16	22
	21	8
	25	18
	30	5
May	4	15
	9	2
	13	12
	17	23
	22	9
	26	20
	31	6
June	4	17
	9	3
	13	14
	18	0
	22	10
	26	21
July	1	7
	5	18
	10	4
	14	15
	19	1
	23	12
	27	22
Aug.	1	9
	5	19
	10	6
	14	16
	19	3
	23	13
	27	23
Sept.	1	10
	5	20
	10	7
	14	17
	19	4
	23	14
	28	1
Oct.	2	11
	6	22
	11	8
	15	19
	20	5
	24	15
	29	2
Nov.	2	12
	6	23
	11	9
	15	20
	20	6
	24	17
	29	3
Dec.	3	14
	8	0
	12	11
	16	21
	21	8
	25	18
	30	4

δ CEPHEI 3

(1^{d} 14½^{h}.)

Month	d	h
Jan.	2	6
	7	15
	13	0
	18	9
	23	17
	29	2
Feb.	3	11
	8	20
	14	4
	19	13
	24	22
Mar.	2	7
	7	16
	13	0
	18	9
	23	18
	29	3
Apr.	3	12
	8	20
	14	5
	19	14
	24	23
	30	8
May	5	16
	11	1
	16	10
	21	19
	27	3
June	1	12
	6	21
	12	6
	17	15
	22	23
	28	8
July	3	17
	9	2
	14	11
	19	19
	25	4
	30	13
Aug.	4	22
	10	7
	15	15
	21	0
	26	9
	31	18

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, „	18 33 „	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1900, inferred	16 27 „	0·1840	67 40

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following Star's name are the Nos. in Catalogue, pp. 24–26.)

U CEPHEI 13 *.

$(2^{d}\ 11^{h}\ 49\frac{1}{2}^{m}.)$

	d	h	m
Jan.	2	21	14
	7	20	54
	12	20	33
	17	20	12
	22	19	51
	27	19	30
Feb.	1	19	9
	6	17	48
	11	18	28
	16	18	7
	21	17	46
	26	17	25
March.	3	17	4
	8	16	43
	13	16	23
	18	16	2
	23	15	41
	28	15	20
April.	2	14	59
	7	14	38
	12	14	17
	17	13	57
	22	13	36
	27	13	15
May.	2	12	54
	7	12	33
	12	12	12
	17	11	51
	22	11	31
	27	11	10
June.	1	10	49
	6	10	28
	11	10	7
	16	9	46
	21	9	25
	26	9	4

	d	h	m
July.	1	8	44
	6	8	23
	11	8	2
	16	7	41
	21	7	20
	26	6	59
	31	6	39
August.	5	6	18
	10	5	57
	15	5	36
	20	5	15
	25	4	54
	30	4	33
Sept.	4	4	13
	9	3	52
	14	3	31
	19	3	10
	24	2	49
	29	2	28
Oct.	4	2	7
	9	1	47
	14	1	26
	19	1	5
	24	0	44
	29	0	23
Nov.	3	0	2
	7	23	41
	12	23	21
	17	23	0
	22	22	39
	27	22	18
Dec.	2	21	57
	7	21	36
	12	21	15
	17	20	55
	22	20	34
	27	20	13

ALGOL 35 *.

$(2^{d}\ 20^{h}\ 49^{m}.)$

	d	h	m
Jan.	1	22	46
	7	16	24
	13	10	1
	19	3	39
	24	21	17
	30	14	55
Feb.	5	8	33
	11	2	11
	16	19	49
	22	13	26
	28	7	4
March.	6	0	42
	11	18	20
	17	11	58
	23	5	36
	28	23	12
April.	3	16	51
	9	10	29
	15	4	7
	20	21	45
	26	15	23
June.	25	20	30
July.	1	14	8
	7	7	45
	13	1	23
	18	19	1
	24	12	39
	30	6	17

	d	h	m
August.	4	59	55
	10	17	32
	16	11	10
	22	4	48
	27	22	26
Sept.	2	16	4
	8	9	42
	14	3	19
	19	30	57
	25	14	35
Oct.	1	8	13
	7	1	51
	12	19	29
	18	13	6
	24	6	44
	30	0	22
Nov.	4	18	0
	10	11	34
	16	5	16
	21	22	53
	27	16	31
Dec.	3	10	9
	9	3	47
	14	21	25
	20	15	2
	26	8	40

λ TAURI 41 *.

$(3^{d}\ 22^{h}\ 52^{m}.)$

	d	h	m
Jan.	4	10	53
	12	8	37
	20	6	21
	28	4	6
Feb.	5	1	50
	12	23	35
	20	21	19
	28	19	3

	d	h	m
March.	8	16	48
	16	14	32
	24	12	17
July.	1	8	2
	9	5	46
	17	3	30
	25	1	15

λ TAURI 41 *.

(*continued*).

	d	h	m
August.	1	22	59
	9	20	44
	17	18	28
	25	16	12
Sept.	2	13	57
	10	11	41
	18	9	26
	26	7	10
Oct.	4	4	54
	12	2	39

	d	h	m
Oct.	20	0	23
	27	22	8
Nov.	4	19	52
	12	17	36
	20	15	12
	28	13	5
Dec.	6	10	50
	14	8	34
	22	6	18
	30	4	3

R CANIS MAJ. 75 †.

$$\left\{\begin{array}{l} 1 = 1^{d}\ 3^{h}\ 16^{m} \\ 2 = 2\ \ \ 6\ \ \ 31 \end{array}\right\}$$

	d	h	m
Jan.	1	5	1
	4	14	48
	8	0	35
	11	10	22
	14	20	10
	18	5	57
	21	15	44
	25	1	32
	28	11	19
	31	21	6
Feb.	4	7	14
	7	16	41
	11	2	28
	14	12	15
	17	22	3
	21	7	50
	24	17	37
	28	3	25
March.	3	13	12
	6	22	59
	10	8	47
	13	18	34

	d	h	m
March.	17	4	21
	20	14	8
	23	23	56
	27	9	41
	30	19	30
April.	3	5	18
	6	15	5
	10	0	52
	13	10	40
	16	20	27
	20	6	14
	23	16	1
	27	1	49
	30	11	36
May.	3	21	23
	7	7	11
	10	16	58
	14	2	45
	17	12	33
	20	22	20
	24	8	7
	27	17	54
	31	3	42

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† *Every third minimum only.*

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 75 †

(continued).

Month	d	h	m
Sept.	1	7	15
	4	17	2
	8	2	49
	11	12	36
	14	22	24
	18	8	11
	21	17	58
	25	3	26
	28	13	33
Oct.	1	23	20
	5	9	8
	8	18	55
	12	4	42
	15	14	29
	19	0	17
	22	10	4
	25	19	51
	29	5	39
Nov.	1	15	26
	5	1	13
	8	11	1
	11	20	48
	15	6	35
	18	16	22
	22	2	10
	25	11	57
	28	21	44
Dec.	2	7	32
	5	17	19
	9	3	6
	12	13	15
	15	22	41
	19	8	28
	22	18	15
	26	4	3
	29	13	50

S Cancri 95.

Month	d	h	m
Jan.	8	20	0
	18	7	38
	27	19	15
Feb.	6	6	53
	15	18	31
	25	6	9
March.	6	17	46
	16	5	24
	25	17	2
April.	4	4	40
	13	16	17
	23	3	55
May.	2	15	33
	12	3	11
	21	14	48
	31	2	26
June.	9	14	4
	19	1	42
	28	13	19
July.	8	0	57
	17	12	35
	27	0	13
Sept.	2	22	44
	12	10	21
	21	21	59
Oct.	1	9	37
	10	21	15
	20	8	52
	29	20	30
Nov.	8	8	8
	17	19	46
	27	7	23
Dec.	6	19	1
	16	6	39
	25	18	17

δ Libræ 165 *.

$(2^d\ 7^h\ 51^m.)$

Month	d	h	m
Jan.	1	7	38
	5	23	21
	10	15	4
	15	6	47
	19	22	29
	24	14	12
	29	5	55
Feb.	2	21	38
	7	13	20
	12	5	3
	16	20	46
	21	12	29
	26	4	11
March.	2	19	54
	7	11	37
	12	3	20
	16	19	2
	21	10	45
	26	2	28
	30	18	11
April.	4	9	53
	9	1	36
	13	17	19
	18	9	2
	23	0	44
	27	16	27
May.	2	8	10
	6	59	53
	11	15	36
	16	7	18
	20	23	1
	25	14	44
	30	6	27
June.	3	22	9
	8	13	52
	13	5	35
	17	21	18
	22	13	0
	27	4	43
July.	1	20	26
	6	12	9
	11	3	51
	15	19	34
	20	11	17
	25	3	0
	29	18	42
August.	3	10	25
	8	2	8
	12	17	51
	17	9	33
	22	1	16
	26	16	59
	31	8	42
Sept.	5	0	25
	9	16	7
	14	7	50
	18	23	33
	23	15	16
	28	6	58
Dec.	2	10	57
	7	2	40
	11	18	22
	16	10	5
	21	1	48
	25	17	31
	30	9	14

U Coronæ 171 *.

$(3^d\ 10^h\ 51^m.)$

Month	d	h	m
Jan.	2	6	2
	9	3	44
	16	1	26
	22	23	8
	29	20	50
Feb.	5	18	32
	12	16	14
	19	13	55
	26	11	38

U Coronæ 171 *

(continued).

Month	d	h	m
March.	5	9	20
	12	7	2
	19	4	45
	26	2	27
April.	2	0	9
	8	21	51
	15	19	33
	22	17	15
	29	14	57
May.	6	12	39
	13	10	21
	20	8	3
	27	5	45
June.	3	3	27
	10	1	9
	16	22	51
	23	20	33
	30	18	15
July.	7	15	57
	14	13	39
	21	11	21
	28	9	3
August.	4	6	45
	11	4	27
	18	2	9
	24	23	51
	31	21	33
Sept.	7	19	15
	14	16	57
	21	14	39
	28	12	21
Oct.	5	10	3
	12	7	45
	19	5	27
	26	3	9
Nov.	2	0	51
	8	22	34
	15	20	16
	22	17	58
	29	15	40
Dec.	6	13	22
	13	11	4
	20	8	46
	27	6	28

U Ophiuchi 216 ‡.

$$\left\{\begin{array}{lll} 1=0^d & 20^h & 8^m \\ 2=1 & 16 & 16 \\ 3=2 & 12 & 23 \end{array}\right\}$$

Month	d	h	m
Jan.	0	14	35
	3	23	6
	7	7	37
	10	16	8
	14	0	39
	17	9	9
	20	17	40
	24	2	11
	27	10	42
	30	19	13
Feb.	3	3	44
	6	12	15
	9	20	46
	13	5	17
	16	13	48
	19	22	19
	23	6	50
	26	15	21
March.	1	23	52
	5	8	23
	8	16	54
	12	1	24
	15	9	55
	18	18	26
	22	2	57
	25	11	28
	28	19	59

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

‡ *Every fourth minimum* only.

Minima of Variable Stars of the Algol Type (continued).

U Ophiuchi 216 ‡ (*continued*).

	d	h	m
April.	1	4	0
	4	13	1
	7	21	32
	11	6	3
	14	14	34
	17	23	5
	21	7	36
	24	16	7
	28	0	38
May.	1	9	9
	4	17	40
	8	2	10
	11	10	41
	14	19	12
	18	3	43
	21	12	14
	24	20	45
	28	5	16
	31	13	47
June.	3	22	18
	7	6	49
	10	15	20
	13	23	51
	17	8	22
	20	16	53
	24	1	24
	27	9	55
	30	18	26
July.	4	2	56
	7	11	27
	10	19	58
	14	4	29
	17	13	0
	20	21	31
	24	6	2
July.	27	14	33
	30	23	4
August.	3	7	35
	6	16	6
	10	0	37
	13	9	8
	16	17	39
	20	2	10
	23	10	41
	26	19	12
	30	3	42
Sept.	2	12	13
	5	20	44
	9	5	15
	12	13	46
	15	22	17
	19	6	48
	22	15	19
	25	23	50
	29	8	21
Oct.	2	16	52
	6	1	23
	9	9	54
	12	18	25
	16	2	56
	19	11	27
	22	19	58
	26	4	28
	29	12	59
Nov.	1	21	30
	5	6	1
	8	14	32
	11	23	3
	15	7	34

Z Herculis 223 ‡.

$$\left\{\begin{array}{l} 1=2^{d}\ 1^{h}\ 16^{m} \\ 2=3\ \ 23\ \ 50 \\ 3=6\ \ 1\ \ 6 \end{array}\right\}$$

	d	h	m
Jan.	1	7	13
	9	6	52
Jan.	17	6	31
	25	6	10

Z Herculis 223 ‡ (*continued*).

	d	h	m
Feb.	2	5	49
	10	5	29
	18	5	8
	26	4	47
March.	6	4	26
	14	4	5
	22	3	44
	30	3	23
April.	7	3	2
	15	2	41
	23	2	21
May.	1	2	0
	9	1	39
	17	1	18
	25	0	57
June.	2	0	36
	10	0	15
	17	23	54
	25	23	33
July.	3	23	12
	11	22	52
July.	19	22	31
	27	22	10
August.	4	21	49
	12	21	28
	20	21	7
	28	20	46
Sept.	7	21	42
	15	21	22
	23	21	1
Oct.	1	20	40
	9	20	19
	17	19	58
	25	19	38
Nov.	2	19	17
	10	18	56
	18	18	35
	26	18	14
Dec.	4	17	53
	12	17	32
	20	17	11
	28	16	51

U^3 Cygni 270 *.

($4^{d}\ 13^{h}\ 45^{m}$)

	d	h	m
Jan.	0	17	58
	9	21	28
	19	0	59
	28	4	29
Feb.	6	7	59
	15	11	29
	24	14	59
March.	5	18	29
	14	21	59
	24	1	29
April.	2	4	59
	11	8	29
	20	11	59
	29	15	29

U^3 Cygni 270 * (*continued*).

	d	h	m
May.	8	18	59
	17	22	29
	27	1	59
June.	5	5	30
	14	9	0
	23	12	30
July.	2	16	0
	11	19	30
	20	23	0
	30	2	39
Aug.	8	6	0
	17	9	30
	26	13	0
Sept.	4	16	30
	13	20	0
	22	23	20
Oct.	2	3	0
	11	6	30
	20	10	1
	29	13	31
Nov.	7	17	1
	16	20	31
	26	0	1
Dec.	5	3	31
	14	7	1
	23	10	31

W Delphini 280 †.

$$\left\{\begin{array}{l} 1=4^{d}\ 19^{h}\ 21^{m} \\ 2=9\ \ 14\ \ 42 \end{array}\right\}$$

	d	h	m
Jan.	1	9	46
	15	19	50
	30	5	53
Feb.	13	15	57
	28	1	57
March.	14	12	4
	28	22	8
April.	12	8	11
	26	18	15
May.	11	4	18
	25	14	22
June.	9	0	26
	23	10	29
July.	7	20	33
	22	6	36
Aug.	5	16	40
	20	2	44
Sept.	3	12	47
	17	22	51
Oct.	2	8	54
	16	18	58
	31	5	2
Nov.	14	15	5
	29	1	9
Dec.	13	11	12
	27	21	16

---

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

Y Cygni 295 ‡.

$1 = 1^d\ 21^h\ 19^m$
$2 = 2\ \ 23\ \ 54$
$3 = 4\ \ 21\ \ 14$

	d	h	m
Jan.	1	7	26
	7	7	15
	13	7	5
	19	6	55
	25	6	45
	31	6	34
Feb.	6	6	24
	12	6	14
	18	6	4
	24	5	53
March.	2	5	43
	8	5	33
	14	5	22
	20	5	12
	26	5	2
April.	1	4	52
	7	4	41
	13	4	31
	19	4	21
	25	4	11
May.	1	4	0
	7	3	50
	13	3	40
	19	3	30
	25	3	19
	31	3	9
June.	6	2	59
	12	2	49
	19	2	38
	25	2	28
	30	2	18
July.	6	2	8
	12	1	57
	18	1	47
	24	1	37
	30	1	27
August.	5	1	16
	11	1	6
	17	0	56
	23	0	46
	29	0	35
Sept.	4	0	25
	10	0	15
	16	0	4
	21	23	54
	27	23	44
Oct.	3	23	34
	9	23	23
	15	23	13
	21	23	3
	27	22	53
Nov.	2	22	42
	8	22	32
	14	22	22
	20	22	12
	26	22	2
Dec.	2	21	51
	8	21	41
	14	21	31
	20	21	20
	26	21	10

S Antliæ 103 §.

	d	h	m
Jan.	0	16	51
	3	22	39
	7	4	27
	10	10	15
	13	16	3
	16	21	51
	20	3	39
	23	9	27
	26	15	15
	29	21	3
Feb.	2	2	51
	5	8	39
	8	14	27
	11	20	15
	15	2	3
	18	7	51
	21	13	39
	24	19	27
	28	1	15
March.	3	7	3
	6	12	51
	9	18	39
	13	0	27
	16	6	15
	19	12	3
	22	17	51
	25	23	39
	29	5	27
April.	1	11	15
	4	17	3
	7	22	51
	11	4	39
	14	10	27
	17	16	15
	20	22	8
	24	3	51
	27	9	39
	30	15	27
May.	3	21	15
	7	3	3
	10	8	51
	13	14	39
	16	20	27
	20	2	15
	23	8	3
	26	13	51
	29	19	39
June.	2	1	27
	5	7	15
	8	13	3
	11	18	51
	15	0	39
	18	6	27
	21	12	15
	24	18	3
	27	23	51
July.	1	5	39
	4	11	27
	7	17	15
	10	23	3
	14	4	51
	17	10	39
	20	16	27
	23	22	15
	27	4	3
Sept.	4	1	39
	7	7	27
	10	13	15
	13	19	3
	17	0	51
	20	6	39
	23	12	27
	26	18	15
	30	0	3
Oct.	3	5	51
	6	11	39
	9	17	27
	12	23	15
	16	5	3
	19	10	51
	22	16	39
	25	22	27
	29	4	15
Nov.	1	10	3
	4	15	51
	7	21	37
	11	3	27
	14	9	15
	17	15	3
	20	20	51
	24	2	39
	27	8	27
	30	14	15
Dec.	3	20	3
	7	1	51
	10	7	39
	13	13	27
	16	19	15
	20	1	3
	23	6	51
	26	12	39
	29	18	27

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‡ Every fourth minimum only.

§ Every tenth minimum only; the others may be found by adding multiples of the period $7^h\ 46^m{\cdot}8$.

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 24–26; M signifies maximum; *m*, minimum.

January.

d	No.
1	121 M
2	252 M
	272 M
	58 *m*
	107 *m*
4	305 M
	74 *m*
5	262 *m*
	226 *m*
6	217 M
	274 M
8	131 *m*
9	307 M
	112 *m*
	278 *m*
10	39 M
	267 M
	304 M
	44 *m*
	77 *m*
11	164 M
	37 M
14	114 M
	303 M
15	248 *m*
16	177 M
	97 M
	156 M
17	161 M
	276 M
	281 M
	239 *m*
	260 *m*
18	200 M
	146 M
19	16 M
22	93 M
	112 M
	60 *m*
	292 *m*
23	306 *m*
24	214 M
	264 M
25	158 M
	11 M
	98 M
	145 *m*
	244 *m*
26	326 *m*
27	199 M
28	201 M
	77 M
	143 *m*
29	136 M
	299 *m*
30	286 M
31	160 M
	163 M

February.

d	No.
1	26 M
	55 *m*
2	83 M
5	290 M
6	157 M
	117 M
	301 *m*
7	243 M
8	136 *m*
	82 *m*
	32 *m*
	29 *m*
9	239 M
	196 M
	71 *m*
12	212 *m*
13	260 M
	112 *m*
15	207 M
	18 *m*
16	46 M
	275 M
	24 *m*
17	119 M
	189 M
19	90 *m*
20	193 M
23	181 M
	116 *m*
	76 *m*
24	273 *m*
25	317 *m*
	77 *m*
26	246 M
27	112 M
	149 M
	87 M
28	174 M
	68 M
	319 *m*

March.

d	No.
2	274 *m*
3	134 M
5	259 M
	239 *m*
6	327 *m*
7	47 M
	234 M
	325 M
	121 *m*
	80 *m*
8	145 M
	109 *m*
11	180 M
	307 *m*
12	7 M

March (*con.*).

d	No.
13	249 M
	168 *m*
	123 *m*
14	133 M
	74 M
	169 M
	8 *m*
	147 *m*
15	23 M
	77 M
	245 *m*
16	3 M
17	84 M
	114 *m*
18	274 M
	188 M
	43 *m*
	228 *m*
	272 *m*
20	19 M
	162 *m*
21	44 M
	112 *m*
	189 *m*
22	38 M
24	226 M
25	218 M
	142 *m*
26	172 M
	40 M
	260 *m*
27	239 M
	213 *m*
	152 *m*
28	113 M
29	283 M
30	205 M
31	131 M
	138 *m*

April.

d	No.
1	299 M
	256 M
	83 *m*
2	268 M
	148 M
	281 *m*
	164 *m*
3	100 M
	112 M
4	293 M
	208 *m*
5	170 M
	241 *m*
	210 *m*
6	287 M

April (*con.*).

d	No.
6	137 M
	183 *m*
	51 *m*
	167 *m*
7	37 *m*
9	177 *m*
10	192 M
	288 M
11	25 *m*
	9 *m*
12	182 M
	77 *m*
13	297 M
15	275 *m*
16	261 M
	56 M
	224 *m*
17	59 M
19	101 M
20	292 M
	282 *m*
	99 *m*
	239 *m*
21	260 M
22	108 M
	21 *m*
23	197 M
24	191 M
25	313 M
	112 *m*
26	145 *m*
27	332 M
	252 *m*
28	31 *m*
29	17 M
	290 *m*
	69 *m*
30	81 M
	77 M
	132 *m*

May.

d	No.
5	111 M
6	306 M
	300 M
	66 *m*
	329 *m*
7	193 *m*
8	61 M
9	112 M
	18 M
	26 *m*
10	127 M
	304 *m*
11	289 M
	274 *m*
12	65 M

May (*con.*).

d	No.
13	322 M
	239 M
14	209 M
15	73 *m*
16	314 M
	20 *m*
18	107 M
	28 M
	24 M
	136 *m*
	134 *m*
19	330 M
20	203 M
	307 M
	189 M
	6 *m*
	161 *m*
21	62 M
	212 M
22	273 M
24	188 *m*
	74 *m*
25	156 *m*
26	178 M
	152 M
27	274 M
	324 *m*
28	255 M
	175 M
	77 *m*
29	22 M
	272 M
30	45 *m*
31	112 *m*

June.

d	No.
1	48 *m*
	276 *m*
	196 *m*
	260 *m*
2	83 M
3	301 M
	47 *m*
	199 *m*
4	259 *m*
	19 *m*
5	311 M
	281 M
6	78 M
	239 *m*
	269 *m*
7	5 M
	145 M
8	110 M
	309 *m*
9	90 M
	248 M

June (*con.*).

d	No.
9	1 M
	14 M
10	116 M
	122 M
	124 M
	29 M
	71 M
11	114 M
12	131 M
13	173 M
	10 M
	112 M
15	82 M
	77 M
	299 *m*
	2 *m*
16	31 M
	328 *m*
17	58 M
18	226 *m*
21	319 M
22	8 M
	189 *m*
23	312 M
24	4 M
25	262 M
26	275 M
	168 M
	172 *m*
27	138 M
	158 *m*
28	228 M
	260 M
	239 M
	177 M
29	207 *m*
	302 *m*

July.

d	No.
4	284 M
	143 M
5	54 M
	315 M
	76 M
6	164 M
	112 *m*
8	37 M
	217 *m*
9	160 *m*
	325 *m*
10	121 M
	118 *m*
	68 *m*
11	12 *m*
	249 *m*
12	125 M
13	251 M

July (*con.*).

d	No.
14	77 *m*
	38 *m*
16	150 M
18	26 M
	326 M
	323 M
19	112 M
	308 *m*
20	296 M
	290 M
	9 M
	303 *m*
	274 *m*
	23 *m*
21	162 M
	307 *m*
22	136 M
	239 *m*
24	132 M
25	271 M
26	134 M
	145 *m*
27	327 M
28	198 M
30	317 M
	83 *m*
31	123 M

August.

d	No.
1	25 M
	77 M
	32 M
	74 M
4	51 M
	141 M
5	43 M
	274 M
	57 *m*
7	18 *m*
8	210 M
	244 M
	260 *m*
9	305 M
	133 *m*
10	264 *m*
	112 *m*
12	49 *m*
	272 *m*
13	245 M
	114 *m*
14	237 M
	55 M
	277 M
	292 *m*
15	213 M
16	199 M
	267 M
17	306 *m*

August (*con.*).

d	No.
18	185 M
19	234 *m*
	281 *m*
20	188 M
	268 *m*
21	52 M
	189 M
	24 *m*
22	3 *m*
23	96 M
	112 M
	275 *m*
24	167 M
	47 M
25	109 M
26	178 M
27	241 M
	257 M
28	298 M
	128 M
	89 *m*
29	161 M
	77 *m*
31	212 *m*

September.

d	No.
1	208 M
	170 *m*
2	31 M
3	80 M
	260 M
	7 *m*
	152 *m*
4	183 M
	226 M
5	53 M
	283 *m*
6	145 M
	239 *m*
7	72 M
	259 M
	56 *m*
	205 *m*
8	136 M
9	66 M
10	60 *m*
11	301 *m*
	121 *m*
12	266 M
14	108 *m*
15	112 *m*
16	77 M
17	176 M
	200 *m*
	148 *m*
19	215 M
	190 M

September (*con.*).

d	No.
19	69 M
20	273 *m*
	177 *m*
21	232 M
	39 M
22	196 M
	147 M
	73 M
	189 *m*
23	146 M
26	164 *m*
28	156 M
	112 M
29	307 M
	239 M
	274 *m*
30	97 M
	83 M
	193 M
	297 *m*
	228 *m*

October.

d	No.
1	279 M
	142 M
2	11 M
	149 M
	37 *m*
	29 *m*
3	8 *m*
4	172 M
6	304 M
7	15 M
8	300 M
9	194 M
	252 *m*
10	134 *m*
11	247 M
	74 *m*
	290 *m*
12	20 M
	248 *m*
13	316 M
	19 M
14	160 M
	77 *m*
	260 *m*
	182 *m*
15	274 M
	202 M
16	224 M
	131 *m*
17	151 M
	197 M
18	38 M
19	207 M
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Maxima and Minima of Variable Stars (continued).

October (con.)			October (con.)			November (con.)			November (con.)			November (con.)			November (con.)			December (con.)			December (con.)			De (
d	No.		d	No.		d	No.		d	No.		d	No.		d	No.		d	No.		d	No.		d
20	112	m	29	18	M	2	152	M	14	239	M	23	93	M	30	289	M	7	50	M	18	134	M	28
	111	m		249	M	3	275	M	15	324	M	24	133	M		16	m		212	M		178	M	
21	65	m		276	M		302	M		242	M	25	112	m				8	21	M		259	m	29
22	325	M		299	m		269	M	16	78	m	26	318	M					112	M	19	74	M	
	281	M	30	230	M	5	22	m	18	68	M		333	M	December.				274	m	20	217	M	
23	180	M	31	218	M		107	m		90	m		282	M					239	m		332	m	30
	272	M		174	m	6	6	M	19	2	M		199	M		214	M	9	177	M	21	81	m	
	239	m					168	m		122	M		76	m		58	m	11	169	M		260	m	
	26	m					62	m	20	24	M	27	83	m		162	m	12	328	M	23	268	M	
24	138	m	November.			7	114	M		17	m	28	252	M	3	59	m		329	M		116	m	31
	287	m				10	292	M		61	m		306	M	4	40	M	15	181	M	24	243	M	
25	145	m	1	77	M		36	M		47	m	29	77	m	5	309	M		3	M		274	M	
26	192	M	2	100	M		260	M	21	45	M		82	m		132	m		193	m		189	m	
	158	M		33	M		98	M		189	M		226	m	6	288	M	16	273	M	26	113	M	
	188	m		112	M	12	246	m	23	46	M		307	m		145	M	17	77	M		136	m	

In the above list the dates given for the Stars indicated by the following numbers are doubtful, as the periods are not known with sufficient accuracy:—

5, 15, 28, 32, 39, 52, 66, 80, 119, 122, 131, 167, 194, 214, 224, 244, 251, 266, 267, 279, 284, 296, 297, 309, 313, 318, 326.

MAXIMA OF U PEGASI.

Jan.			April.			June.			August.			Oct.			Dec.		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
0	3	36	3	20	19	2	19	29	1	18	39	4	11	46	3	10	56
3	21	33	7	14	15	6	13	26	5	12	36	8	5	43	7	4	53
7	15	30	11	8	12	10	7	23	9	6	33	11	23	40	10	22	50
11	9	27	15	2	9	14	1	19	13	0	30	15	17	37	14	16	47
15	3	24	18	20	6	17	19	16	16	18	27	19	11	34	18	10	44
18	21	21	22	14	3	21	13	13	20	12	23	23	5	31	22	4	41
22	15	18	26	8	0	25	7	10	24	6	20	26	23	27	25	22	38
26	9	14	30	1	57	29	1	7	28	0	17	30	17	24	29	16	35
30	3	11							31	18	14						
Feb.			May.			July.			Sept.			Nov.					
2	21	8	3	19	54	2	19	4	4	12	11	3	11	21			
6	15	5	7	13	51	6	13	1	8	6	8	7	5	18			
10	9	2	11	7	47	10	6	58	12	0	5	10	23	15			
14	2	59	15	1	44	14	0	55	15	18	2	14	17	12			
17	20	56	18	19	41	17	18	51	19	11	59	18	11	9			
21	12	53	22	13	38	21	12	48	23	5	55	22	5	6			
25	8	50	26	7	35	25	6	45	26	23	52	25	23	3			
			30	1	32	29	0	42	30	17	49	29	16	59			

Every twentieth minimum is given; the others may be found by adding *multiples of the period* $4^h\ 29^m\ 8^s$.
The minimum takes place $2^h\ 15^m$ after the maximum.

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1890+.	Observer.
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½ ; 7½	336·7	1·68	8·12	M.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½ ; 6½	149·6	0·38	9·01	Ls.
3.	β 395	0 32·2	−25 19	6·5 ; 6·7	280·4	0·38	8·59	Ait.
4.	β 865	0 38·2	+42 40	8 ; 8½	192·0	1·48	8·95	Ls.
5.	β 866	0 39·1	+42 34	9 ; 9	74·8	1·44	9·01	Ls.
6.	Σ 60, η Cassiop.	0 43·0	+57 17	3½ ; 7½	217·2	5·27	9·15	M.
7.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	330·2	0·42	7·88	Ls.
8.	Σ 73, 36 Andromedæ	0 50·1	+23 2	6 ; 6½	17·1	1·07	8·84	M.
9.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½ ; 7	354·0	1·66	6·91	M.
10.	β 1000	1 30·3	−30 26	8 ; 13	356·4	1·44	1·84	β.
11.	Σ 138	1 30·8	+ 7 7	7 ; 7½	37·5	1·51	5·92	Ls.
12.	p Eridani	1 35·7	−56 0	6 ; 6	224·0	7·35	4·96	S.
13.	Σ 202, α Piscium	1 56·8	+ 2 17	4 ; 5	320·2	2·78	5·85	C.
14.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½ ; 5½	65·4	10·30	8·68	Ls.
15.	Σ 228	2 7·3	+56 59	6½ ; 7½	80·0	0·43	9·89	Ls.
16.	Σ 305	2 41·8	+18 58	7½ ; 8	317·2	3·02	8·11	Ls.
17.	Σ 333, ε Arietis	2 53·5	+20 56	5½ ; 6	203·3	1·11	9·16	Ls.
18.	β 525	2 53·5	+21 13	7½ ; 7½	132·6	0·17	9·15	Ls.
19.	β 530	3 9·1	+22 33	9½ ; 10	193·3	2·23	8·10	Ls.
20.	Σ 412, 7 Tauri	3 28·5	+24 8	6½ ; 7	9·6	0·21	9·15	Ls.
21.	Σ 422	3 31·8	+ 0 16	6 ; 8	247·1	6·19	5·90	C.
22.	Σ 425	3 33·8	+33 49	7½ ; 8	91·2	2·51	9·11	M.
23.	OΣ 531	4 0·5	+37 48	7 ; 7½	131·0	1·96	9·19	M.
24.	Σ 518, o² Eridani	4 10·6	− 7 49	8½ ; 10½	87·4	2·32	5·91	C.
25.	β 883, AB	4 45·6	+10 54	7½ ; 8	49·6	0·36	9·09	Ls.
26.	,, AC	……	……	7½ ; 10	157·5	18·36	9·09	Ls.
27.	Σ 535	4 17·7	+11 9	7 ; 8	329·3	1·58	8·08	Ait.
28.	Aldebaran	4 30·2	+16 9	1 ; 14	109·7	31·30	8·10	Ait.
29.	OΣ 98, ι Orionis	5 2·4	+ 8 22	5½ ; 7	179·1	1·06	8·17	M.
30.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	258·8	3·50	5·16	M.
31.	Σ 749	5 30·9	+26 53	6½ ; 7½	173·8	0·82	9·18	Ls.
32.	Σ 932	6 28·5	+14 49	8 ; 8	328·1	2·34	3·18	Ls.
33.	OΣ 149	6 30·0	+27 22	7 ; 9	277·8	0·68	9·11	Ls.
34.	Σ 948, 12 Lyncis AB	6 37·3	+59 33	6 ; 6½	119·5	1·67	7·43	M.
35.	Sirius	6 40·6	−16 34	1 ; 10	161·9	4·22	8·73	Ait.
36.	Hough 28	6 50·0	+27 8	9½ ; 9½	255·7	6·45	7·14	Ho.
37.	Σ 1037	7 6·6	+27 24	7 ; 7	305·0	0·92	9·06	Ls.
38.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	109·2	1·50	7·25	M.
39.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	205·3	6·89	6·08	C.
40.	Σ 1074	7 15·4	+ 0 36	7½ ; 8½	133·8	0·68	6·18	Ls.
41.	OΣ 175	7 27·7	+31 10	6 ; 6½	334·5	0·61	9·15	Ls.
42.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	223·9	5·76	9·07	Ls.
43.	Procyon	7 34·0	+ 5 29	1 ; 8½	326·5	4·83	8·21	Bd.
44.	Σ 1187	8 3·2	+32 31	7 ; 8	224·9	2·17	9·12	Ls.
45.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½ ; 7½	8·2	1·09	9·12	Ls.
46.	,, ,, AC	……	……	5½ ; 6½	114·0	5·68	9·12	Ls.
47.	,, ,, BC	……	……	7½ ; 6½	127·2	5·78	9·12	Ls.
48.	β 205	8 28·8	−24 16	7 ; 7	236·5	0·75	8·25	Ait.
49.	β 208	8 34·6	−22 20	7 ; 8	76·2	0·45	8·29	Ait.
50.	Σ 1273, ε Hydræ	8 41·5	+ 6 50	4 ; 7	233·4	3·22	7·31	M.
51.	Σ 3121	9 11·9	+29 0	7½ ; 7½	198·6	0·83	8·29	M.
52.	OΣ 201	9 18·0	+28 20	7 ; 11	222·4	1·24	7·31	Ls.
53.	Σ 1348	9 19·2	+ 6 50	7½ ; 7½	322·0	1·77	6·20	Ls.
54.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½ ; 7	111·7	0·78	8·91	Ait.
55.	Σ 1389	9 45·3	+27 29	8 ; 9	309·8	2·20	9·29	Ls.
56.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½ ; 6	91·2	0·48	8·91	Ait.
57.	OΣ 215, P. x. 23	10 10·9	+18 15	6½ ; 7	208·5	0·87	8·22	Ait.
58.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	115·6	3·63	6·31	Ls.
59.	OΣ 218	10 22·1	+ 4 5	7 ; 9	74·0	1·04	6·30	Ls.
60.	OΣ 224	10 34·5	+ 9 22	7 ; 9	317·4	0·58	7·34	Ls.

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1890+.	Ob-server
		h m	° ′		°	″		
61.	OΣ 229	10 42·3	+41 38	6 ; 7	321·1	0·76	9·39	La.
62.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	154·5	2·04	9·35	La.
63.	Σ 1536, ι Leonis	11 18·7	+11 5	4½ ; 7½	59·6	2·53	6·34	La.
64.	OΣ 235	11 26·7	+61 38	5½ ; 7	109·6	0·68	8·91	Ait.
65.	β 603	11 53·5	+14 50	6½ ; 10	323·2	0·98	8·30	La.
66.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½ ; 8	219·8	1·38	9·29	La.
67.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	150·2	5·67	8·35	M.
68.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	77·0	1·08	9·34	La.
69.	β 112	12 55·2	+19 1	9 ; 9½	294·6	2·12	9·37	La.
70.	OΣ 266	13 23·6	+16 14	7 ; 8	341·5	1·76	8·42	M.
71.	β 933	13 30·0	+33 39	8 ; 8½	29·5	2·32	8·37	Ait.
72.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	135·3	1·16	8·13	Ait.
73.	Σ 1785	13 44·5	+27 29	7½ ; 8	270·5	1·57	8·44	M.
74.	Σ 1788	13 49·7	− 7 34	6½ ; 7½	75·5	2·60	6·38	M.
75.	Σ 1819	14 10·3	+ 3 36	7½ ; 7½	2·5	1·21	9·30	La.
76.	Σ 1865, ζ Boötis	14 36·3	+14 10	4 ; 4	352·4	0·10	8·44	La.
77.	Σ 1877, ε Boötis	14 40·6	+27 30	3 ; 6½	326·1	2·70	6·46	M.
78.	Σ 1879	14 41·3	+10 4	8 ; 8½	139·5	0·46	8·44	La.
79.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	208·5	2·52	9·44	M.
80.	β 31	14 47·8	+19 9	8½ ; 9½	196·6	1·38	9·44	La.
81.	OΣ 287	14 47·9	+45 20	7 ; 8	316·3	0·85	6·68	M.
82.	β 239	14 52·7	−27 15	5½ ; 6	316·5	0·99	8·28	Ait.
83.	β 350	15 8·2	−27 10	7 ; 8½	160·2	1·15	2·41	L.
84.	Σ 1932, 1 Cor. Bor.	15 14·0	+27 13	6½ ; 7	148·6	0·71	9·42	La.
85.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	172·0	0·58	9·34	La.
86.	Σ 1938, μ² Boötis	15 20·7	+37 22	6½ ; 7½	74·7	1·00	9·64	M.
87.	OΣ 298	15 32·4	+40 8	7 ; 7½	178·0	0·90	8·53	La.
88.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	117·0	0·43	9·42	La.
89.	OΣ 303	15 56·2	+13 33	7 ; 7½	144·9	0·71	9·45	La.
90.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	215·3	0·94	6·46	M.
91.	Σ 2032, σ Coronæ	16 11·1	+34 7	6 ; 6½	210·6	4·16	6·45	La.
92.	Σ 2055, λ Ophiuchi	16 25·8	+ 2 12	4½ ; 5½	54·8	1·07	9·44	La.
93.	Σ 2084, ζ Herculis	16 37·5	+31 47	3 ; 6½	264·5	0·60	9·45	La.
94.	Σ 2114	16 56·5	+ 8 36	6½ ; 8	160·1	1·06	9·45	La.
95.	Σ 2140, α Herculis	17 10·1	+14 30	2½ ; 6	115·9	4·78	9·57	La.
96.	β 629	17 15·0	+32 10	8½ ; 8½	340·4	1·08	9·50	La.
97.	Σ 2173, 221 Ophiuchi	17 25·5	− 0 59	6 ; 6	331·5	0·98	9·57	M.
98.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	257·1	1·93	7·66	M.
99.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	260·2	1·66	9·54	La.
100.	OΣ 358	18 31·3	+16 54	6½ ; 7	193·5	1·85	8·62	M.
101.	β 648	18 54·2	+23 39	6 ; 9	222·4	0·96	9·44	La.
102.	γ Coronæ Aust.	19 0·0	−37 12	5½ ; 5½	148·5	1·68	8·46	Ait.
103.	β 248	19 13·4	+22 51	6 ; 9	129·6	1·84	6·86	M.
104.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	303·4	1·50	7·53	M.
105.	Σ 2695	20 27·7	+25 28	6 ; 8	78·5	0·96	8·71	M.
106.	β 151, β Delphini	20 32·7	+14 15	3½ ; 5½	6·5	0·60	9·77	La.
107.	Σ 2744	20 57·8	+ 1 9	6½ ; 7½	167·0	1·42	9·83	La.
108.	Σ 2799	21 23·9	+10 38	7 ; 7½	119·1	1·47	8·83	M.
109.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	121·5	2·39	7·79	M.
110.	Σ 2909, ζ Aquarii	22 23·7	− 0 32	4 ; 4	322·7	3·31	6·86	M.
111.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	221·7	1·15	7·94	M.
112.	Hough 198	23 11·6	+16 12	8 ; 8½	32·0	2·05	4·76	Ho.
113.	β 80	23 13·2	+ 4 53	8 ; 8½	343·1	0·62	8·78	Ait.
114.	β 720	23 29·0	+30 46	6 ; 6	160·9	0·48	9·77	La.
115.	Σ 3050	23 54·6	+33 10	6 ; 6½	212·4	2·55	8·91	Ait.

In the column headed "Observer," the following abbreviations are used:—

L. *Leavenworth.*	M. Maw.	S. Sellors.	Bd. Barnard.
La. *Lewis.*	Ait. Aitken.	C. Collins.	Ho. Hough.
β. *Burnham.*			

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1899 December 8.

Prof. G. H. DARWIN, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and F. W. DYSON, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. 64 presents have been received since the last Meeting. Those which call for special mention are twelve volumes of 'Bulletins' and 'Mémoires' from the Brussels Academy of Sciences, and a photograph of the new reflecting telescope at Potsdam, which will be passed round for Fellows to see.

Capt. W. Noble. I should like to ask, in the interest of the Fellows generally, why the new number of the 'Monthly Notices' is not yet out.

Mr. Newall. I must express regret that it is not yet distributed. It has been rather a heavy number to deal with, and I can only assure Captain Noble that we are pressing on with it as quickly as we can, and I hope the number will be in the hands of Fellows before very long.

Capt. Noble. I have another small question to ask of the Secretaries, and that is, whether notices of our Meetings are sent to the newspapers. Most of the morning papers have a diary of the day's Meetings: I bought the 'Standard' to read in the train this morning, but I did not see a syllable about our Meeting to-night. I think the Meeting of the Geographical Society was announced, but I do not see that our Meetings are mentioned in these diaries at all.

Mr. Newall. So far as I am aware, the only notice of our Meetings which appears in the papers is one that occurs in 'Nature.' It does not appear in the daily papers.

Capt. Noble. But they used to be mentioned, or I should not mention it. It is rather important that we should not hide our light under a bushel.

On the motion of *Mr. C. Thwaites*, seconded by *Mr. Adams*, Mr. Inwards, Mr. Gibbs, and Mr. Lewis were appointed Auditors of the Treasurer's accounts for the year 1899.

The President. At our last Meeting there were some papers by *Prof. Barnard* which time did not permit us to consider. I will ask the Secretary to tell us something about them.

Mr. Newall said that Prof. Barnard had recently sent to the Society three papers. One of them related to the probable motion of the annular nebula in Lyra; a second one was on the subject of the exterior nebulosities of the Pleiades, with a drawing from different photographs, and on the appearance of the involved nebulosities of the cluster as seen through the 40-inch refractor at the Yerkes Observatory; and the third one on the diameter of Vesta. Mr. Newall proposed first to give an account of the paper relating to the nebula in Lyra. Prof. Barnard began by recalling Prof. Hall's observations of small stars made in 1877 July and August, and gave a list of measurements of 12th mag. stars round the nebula. He pointed out that in 1891 Mr. Burnham measured the central star with the 36-inch refractor of the Lick Observatory. [Diagram on blackboard drawn by Mr. Wesley.] Mr. Burnham's measures were very consistent, and undoubtedly the best that could be made of such an object. The central star was well seen. Prof. Barnard gave the measures of the nucleus made by Mr. Burnham. It was to be regretted that such measures were not followed up by other observers where a micrometer was available. In 1891 for the first time they had the position of the nucleus fixed definitely for reference in regard to future observations. Prof. Barnard stated that he had been remeasuring some of the nebulæ and the measures were in close agreement with those of Mr. Burnham. During 1898–99 Mr. Barnard measured the position of the nucleus, with reference to star "*a*," with the 40-inch. The nucleus was distinctly seen and permitted fairly accurate estimates to be made. He had measured it with reference to the stars "*a*," "*b*," and "*c*," and found that his measures of the nucleus and of the star "*a*" differed from Mr. Burnham's by 1° in angle and 1″ in distance. That was a small distance, but it was worthy of consideration when two observers were so consistent as they always were in measures like these. Mr. Barnard was therefore under the impression that this difference was a real one, and the question was whether it was due to the star "*a*" or the nucleus, and he had made observations to decide the stability of "*a*." In comparing these measures with Mr. Hall's measurements made 22 years ago there appeared to be some change in some of the stars, but there was no very decided motion in star "*a*." "*f*" appeared to be decidedly in motion when compared with "*a*," and he found some possible motion in star "*d*." The measures of "*a*" and "*c*" stars on opposite sides of the nucleus showed that the discrepancy in Profs. Barnard's and Burnham's measures of the nucleus could not be due to any motion in "*a*." Was the central star a real nucleus, or was it an accidental projection? Prof. Barnard supported the view that the star was part of the nebula and not a star seen in the same line of sight. It was almost certain if the motion was found to exist in the central star that it was the motion of the nebula itself. He should be glad to see measures made of any existing photographs of the nebula, and he hoped that this paper would call attention to the importance of securing further accurate data. Prof. Barnard gave the measures secured in 1893–94 of the diameter of the ring nebula itself, and of the position-angle of the major axis. He then dealt with a very interesting set of observations in which he had used the achromatic aberration of the telescope to test whether the supposed nucleus was a nebula or a separate star seen in the line of sight of the nebula. In course of observation of the planetary nebulæ he had made some experiments with the 40-inch refractor, using a magnifying-power of 1300 diameters, by examining the circular spectra of the nucleus, to determine whether the nucleus was of the nature of a star or more closely related to the planetary nebulæ, and he gave the differences in the focus, which were rather instructive from the point of view of chromatic aberration of such a big telescope as the 40-inch refractor. Prof. Barnard had observed six planetary nebulæ and found that the nucleus came to a focus 0·06 inch further out than a star, and that the nebula came to a focus 0·26 inch further out than the star. In observing the ring nebula in the same way, he found that the central star was more nearly in focus with the nebula than in the case of the other six nebulæ, and he concludes that it is to be regarded as part of the nebula itself. This application of the difficulties which were involved in the use of a so-called achromatic telescope in observations of this kind—the use of the difference of the focus — was an instance of the versatility of Prof. Barnard in making the best of a bad job.

Mr. Newall said that the second of Prof. Barnard's papers dealt with the exterior nebulosity of the Pleiades. Prof. Barnard had had a drawing made from various photographs taken by himself and other observers of the nebulosities, and it was hoped that it would be possible to reproduce it in the 'Monthly Notices.'

Dr. Rambaut. I should like to ask how these measures of the ring nebula were made. Were they made from a photograph or directly in the telescope?

Mr. Newall. Directly in the telescope with the micrometer.

Dr. Rambaut. I think, as Prof. Barnard says, this is an object which lends itself particularly well to photographic measures. I was greatly struck by what Prof. Barnard said as to the nature of the nucleus. Those who have photographed this object must be persuaded that the central star is part and parcel of the ring nebula. As the exposure of the plate is continued—that is to say, if exposures of different length are given—it will be seen how the nucleus seems to spread and fill up the interior of the ring. I should like to know whether Prof. Barnard's measures are so continuous that they might be used for parallax investigation.

Mr. Newall. From a hasty look over the papers it seems clear, with reference to the last question, that they would hardly be available. They were nearly all made in July and August, and there are no observations made 6 months afterwards.

A Fellow. What does he make the major axis of the Ring Nebula?

Mr. Newall. The outer major diameter he makes to be 80″·89 and the position-angle 65°·43.

The President. Do I understand that Prof. Barnard uses the focus-reading of his telescope to measure the kind of light that is emitted?

Mr. Newall. Yes.

The President. That, I take it, is altogether an original idea. Has anyone tried it before?

Mr. Newall. So far as I know, the first instance of observation of this kind being made was in the case of Nova Aurigæ. I believe I myself may take credit for having made that first observation when I found that the Nova came to a focus different from that which was suitable for neighbouring stars. No actual measures were made then, but it was realized that the appearance of nebulosity around the Nova when the telescope was focussed for stars was the result not necessarily of nebulosity around the Nova, but rather of aberration of the light coming from the Nova when the surrounding stars were most distinctly focussed. But it must be pointed out in that connection that the achromatic refractor was a most unsuitable instrument to use for the settlement of that question, inasmuch as there were two possible explanations of the appearance of nebulosity.

The President. Then does not that use of the telescope demand a new micrometer, because it is not usual to know the position of the eyepiece for focus with the closest accuracy?

Mr. Newall. In many cases there is a focussing scale on the telescopes of modern observatories. I think it is generally put on for the guidance of the observers in setting, rather than with any idea of using it as a micrometer.

Mr. R. Cooper. I should like to ask what interval of time elapsed between the two measurements of the position for focus, for the star and for the nebula respectively, because it seems to me that the expansion of the tube of the instrument in this interval might make results so obtained very misleading.

Mr. Newall. I think that is a point which Prof. Barnard is very much alive to, for in his second paper relating to the nebulosity round the Pleiades he has said he has been measuring the difference to which the last speaker has referred.

Capt. Noble. Were these measures made with the filar micrometer?

Mr. Newall. Yes.

Capt. Noble. I ask the question because anybody who has made any measures with the filar micrometer must know that there is a great deal of what you may call personal equation or idiosyncrasy in those measures. You rarely find two men who make exactly the same measure with this instrument, and when we are considering small quantities it is much more satisfactory if the measures are made by one man, so as not to have to compare measures made by one man with those made by another.

Mr. Dyson. Is your point the difficulty of comparing Prof. Hall's measures with Prof. Barnard's?

Capt. Noble. Yes.

Mr. Seabroke. What is the distance of the nucleus from star "*a*"?

Mr. Newall. 60".

Mr. Seabroke. And there is only a difference of 1"?

Mr. Newall. Yes. I think it might give Mr. Seabroke an idea if I read out the five observations on different nights of the nucleus of the star "*a*" by the same person.

Mr. Seabroke. That does not affect the issue. The issue is whether certain persons give greater or less measures of the same object. It is well known that there may be such a discrepancy.

Mr. Newall. In that connection it should be pointed out that Prof. Barnard and Prof. Burnham were working together and comparing notes constantly at the Lick Observatory, and Prof. Barnard made measures which led him to think there is a real difference.

Mr. Lewis. I rather take objection to what Mr. Seabroke says: 1-60th of any measure is not necessarily a very small quantity; a 60th of a second of arc is small, but when we come to large quantities like 60", 1-60th is a large quantity. In the measures of the diameter of Jupiter or of the Rings of Saturn you will not find any difference in Prof. Barnard's measures anything like 1" in those large quantities.

Mr. Seabroke. I have always found great difficulty in measureing stars of 60" and more apart. It is almost impossible to keep the eye looking at both at the same time. You must go from one to the other, and in this short interval a shift takes place. When you have the stars close together you can see them both at once and it is easy to measure them, but when you have them far apart it is not easy to measure them.

Mr. Lewis. That depends upon the driving-clock. They have a very good driving-clock at Yerkes and they can trust their instruments for some seconds. I am aware that every double-star observer does not work under such good conditions; but we are talking, in this instance, about Profs. Barnard and Burnham, which is a totally different thing; and as for comparison of one with the other, I understood that Prof. Barnard said he had measured for comparison a very large number of nebulæ also measured by Prof. Burnham.

Mr. Newall. That is so. May I read the paragraph in which Prof. Barnard deals with this? [Read paragraph.] I am under the impression that this apparent change is a real one, and the question is whether it is due to a change in the star "a" or the nucleus.

The President. I am sure the Society will agree with me that it is a great honour that so distinguished an astronomer as Prof. Barnard should send his papers to us.

Mr. W. Ellis then read a paper on the Relation between Magnetic Disturbance and the Period of Solar-spot Frequency. He said that comparisons of this kind had been before made, but not, so far as he knew, for so long a period, certainly not with such complete material and not in the same way. He pointed out that magnetic variations include principally two things—the solar diurnal variation (a local phenomenon) and the apparently irregular magnetic disturbance. But disturbance is not always present, some days are altogether free therefrom, showing only the diurnal variation, but on most days disturbance to a lesser or greater extent exists. In the ordinary Greenwich tabulation all days are used excepting only some of extreme disturbance; thus many days showing disturbance become included. The diurnal range of magnetic declination and horizontal force formed from these tabulations is found (Proc. Roy. Soc. vol. 63, p. 64) to vary both in period and magnitude in close accordance with the variation of sun-spot frequency. It was also shown that, if days of quiet magnetism only are employed, days free from disturbance, the variation of the diurnal range with sun-spots remains of the same character as before. Existing thus in a similar marked degree, whether days of disturbance are or are not included, the variation observed is essentially that of the undisturbed solar diurnal range. The paper now presented takes up the matter here. It considers the question (since it is established that the diurnal range, apart from magnetic disturbance, varies with sun-spot frequency) of the relation of magnetic disturbance itself, apart from diurnal range, to sun-spot frequency. To determine this the Greenwich records for the years 1848 to 1897 were divided into five classes according to intensity of disturbance—Class I. containing days without disturbance, and Classes II., III., IV., and V. days of minor, moderate, active, and great disturbance respectively. The number of days in each class being tabulated, the whole is graphically compared with Wolf's sun-spot curve. Great and active disturbance rises and falls with greater or less sun-spot frequency, moderate disturbance in a less-marked degree, minor disturbance showing little sympathetic variation, whilst days of no disturbance become considerably increased at the sun-spot maxima of 1856 and 1879, showing the marked magnetic quiet existing at these two epochs. The same material has been used to show the seasonal variation of magnetic disturbance, on the aggregate of the fifty years employed. Here the question of sun-spot variation does not enter, there being no annual period therein, as is shown. Disturbance is found to be more active in spring and autumn than in summer and winter, the dying-down of great and active disturbance at the solstices being accompanied by increased minor disturbance, and increase of days of no disturbance; that is, disturbance then sinks to a lower level. Reference is made to some previous discussions of the question of the seasonal variation. The novel point in this paper is conceived to be the separation of disturbance into classes according to intensity thereof. Mr. Ellis gives a summary of results to which this and preceding papers lead, and expresses his grateful thanks to the Astronomer Royal for permission to make use of the Greenwich magnetic records, not only for the purposes of the present paper, but also for others that have preceded it.

During the reading of this paper *Prof. Darwin* left the Chair, which was occupied by *Captain Noble* for the remainder of the Meeting.

The Chairman. I am sure Fellows will thank Mr. Ellis for the very remarkable paper he has read. There is an old story told of an undergraduate coming out of the Senate House at Cambridge having been asked if he had proved a certain proposition and replying that he could not exactly say that he had proved it, but he hoped he had made it appear very probable to the examiner. I think Mr. Ellis has done more than that, because he has shown that some connection exists between sun-spots and magnetism, which was doubted by so eminent an authority as Sir George Airy.

Mr. Dyson. Mr. Ellis's paper consists of a discussion of a very large amount of statistics, and perhaps Fellows generally hardly realize what difficulty there is in the accurate and proper treatment of statistics. It is said that by statistics you can prove anything, and, of course, you can if the statistics are not treated properly; and no doubt it was the ill-treatment of statistics which led Sir George Airy 25 years ago to doubt the connection between sun-spots and terrestrial magnetism, and I think that Mr. Ellis had the honour of convincing so eminent a sceptic as Sir George Airy. There are many difficulties in the subject which require great skill and an immense amount of work to eliminate so as to arrive at some satisfactory result. One point I suppose most of the Fellows will know of, and that is one which Mr. Ellis did not draw attention to himself, so perhaps I might be allowed to do so; and that is, that the earlier work referred to at the beginning of his paper is, I believe, mostly Mr. Ellis's own work which was published in the 'Philosophical Transactions' 20 or 25 years ago. It is a subject we have not often had here, the connection between magnetism and the sun-spots. It is an exceedingly interesting and difficult subject, and we are all deeply indebted to Mr. Ellis for his paper.

Mr. Dyson then exhibited two photographs taken with the reflecting telescope at the Royal Observatory, Greenwich. He said that, owing to a bad cold, the Astronomer Royal was not able to be present that evening. The photographs were taken with the 30-inch reflector of the Thompson Equatorial, the focal length of which is 11 feet $4\frac{1}{2}$ inches. At Greenwich they did not lay themselves out for this work, but as the photographs were interesting, the Astronomer Royal thought the Fellows would like to see them. [Pictures were shown of the Orion Nebula taken with an exposure of $2\frac{1}{4}$ hours and of the Pleiades with one hour exposure.]

The Chairman. Astronomical photography seems to have made the most stupendous strides within the memory of everyone here this evening. I am old enough to remember when, as a young man, I saw the late Dr. De la Rue's pictures of the Moon taken on wet collodion plates, and they were thought to be most marvellous. We little thought in those days what photography would grow to. One thing that ought to strike us in the first picture, but perhaps rather fails to do so, is the stupendous scale upon which these nebulæ must be built. The millions and billions of cubic miles that this matter must occupy in space, even if it has measurable parallax at all, must be enormous. Attention is called to that filamentous appearance as strongly resembling that visible in the solar corona. Whether they have a common cause or not, it would be difficult or impossible to say, or any analogous cause, but as to their optical similarity there can be no doubt whatever. I am sure our thanks are due to Mr. Dyson for showing us these photographs.

Mr. Newbegin. It is very gratifying to feel that all the best instruments are not gone to America. We certainly have here photographs of exceeding beauty, very similar to those taken by an instrument recently gone to America. With regard to the photograph of the Pleiades, it was beautifully depicted on the screen; in fact both pictures are very true and beautiful.

Dr. Rambaut. Might I ask whether in the negative it is possible to trace the connection between the lower nebula and the great nebula? I am unable to do it from where I am sitting. I should like to ask Mr. Dyson whether the photograph was not taken in the principal focus of the telescope. I understood him to speak of it as a Cassegrain.

Mr. Dyson. I spoke of it as Cassegrain to draw attention to the point that a part was cut out of the mirror, with reference to the form of the images at some distance from the centre of the field, but it was taken at the principal focus. I should not like to answer the other question without examining the negative more carefully *.

The Chairman. This may be a fitting time to throw upon the screen a drawing which has been made by Mr. Wesley from a photograph by Prof. Barnard, of the nebulosity surrounding the Pleiades.

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* Examination of the negative shows clearly how the two nebulæ are joined.—[F. W. D.]

The photograph was thrown on the screen.

The Chairman. The interest of this picture seems to me to reside in the fact that it shows us how these groups of stars may have a physical connection. They may form some stupendous system. The apparent figures exhibited by stars in the sky are in general fortuitous, but a group like the Pleiades, involved in nebulosity, which extends to some distance around it, seems to indicate that it actually forms of itself a something analogous to our solar system on a large scale.

The Chairman. I will ask Mr. Dyson to tell us something about the Leonids.

Mr. Dyson. I have been asked that question before. Personally I saw nothing. 20 meteors were observed on Nov. 16 (civil) between the hours of 4.30 and 4.40, and 5.30 and 6.15. Thirteen of these meteors conformed to the Leo radiant. On the night of Nov. 14–15, continuous watch was kept, but dense fog and clouds prevented any observations being made. At the Greenwich Observatory arrangements were made to photograph the meteors, but, owing to the weather, no result was obtained. It is a little interesting to compare the number of meteors seen about this date in other years, and a table has been drawn up. This year on Nov. 16th in one hour 20 meteors were seen, of which 13 were Leonids. Last year it was cloudy on the 15th and 16th. On Nov. 14th, 1897, in two hours 14 meteors were seen, 8 being Leonids; on 1896, Nov. 12–13, in 2 hours 8 meteors were seen, of which two seemed to be Leonids. In 1894, in 2 hours 19 meteors were seen, 7 being Leonids; in 1893 on Nov. 12–13, in 3 hours 21 were seen, of which 4 were Leonids. In 1887, no Leonids were seen.

Dr. Rambaut. I am afraid I must preface the Report of what we did at Oxford with something in the nature of an apology. It was not our fault, we did our best. At the Radcliffe Observatory preparations were made for observing the Leonid shower. Our attention was chiefly directed to an attempt to photograph the meteor-trails and to an enumeration of the numbers we might see. In order not to distract our attention over too many subjects, I decided after some hesitation not to attempt any eye-and-hand tracking of the meteor-trails, because it seemed to me, from the recent photographs of Prof. Pickering and others, that a few photographic trails would give a better determination of the radiant-point and that the amount of work entailed in jotting down the meteor-tracks might be more advantageously directed to other lines. For the photographic work we proposed to use a Ross portrait-lens of 4½ inches aperture and 18 inches focal length. Though we got some clear weather no meteors were bright enough to leave any trace upon the plates. Mr. Wickham and Mr. Robinson undertook to count the meteors, and in anticipation of a large shower we had made arrangements to record the time of each meteor on the chronograph, but unfortunately the richness of the display fell so far short of expectation that this was not done. Our watch began before midnight on the 14th. It was cloudy up to 14.30. At that time the clouds in the East lifted, and it remained fine from then until sunrise. I have given the number of meteors observed in each quarter of an hour beginning at $14^h\ 45^m$. They were 5, 2, 2, 3, 4, 0, 3, 3, 5, 4, 8, 9, 5, 6, 1. That brought us up to ½ past 6. Only 60 Leonids were recorded, of which 37 were noted by one observer and 35 by the other. There were several meteors not belonging to the Leonid radiant. The increasing numbers towards the end of our watch may probably be due to the fact of the waning moonlight as the moon approached the horizon. There was one remarkable meteor which was observed by all four observers. It passed from Leo to midway between Polaris and Capella. It left a bright trail which almost disappeared after 2 or 3 seconds, then a portion of the trail reappeared and quickly changed its form. After a little while it bent in the middle and seemed to divide into two straight portions at an obtuse angle to one another. It eventually broke into two portions and faded away. On the morning of Nov. 16th a watch was again instituted, but the sky remained overcast from midnight until after 5 o'clock. At 5.30 the clouds began to part widely enough to reveal whole constellations at a time, so that the observers were able to see the whole of Leo and sometimes Ursa Major and Orion. These openings were enough to give the idea that the radiant-point was very inactive. Then the sky became overcast and remained so until sunrise. This report may prove valuable as supplementing what was done at Greenwich. On the 15th it was clear with us, and on the 16th, when we were not able to see anything, it was partially clear at Greenwich.

Mr. Newall said that 3 other communications had been received—one from Oxford, one from Durham, and one from Edinburgh. The one from Oxford was by Prof. Turner. He said that Mr. Bellamy had seven cameras ready with four in reserve, and some plates were exposed on Nov 14th but without result. Three observers kept a regular watch in the sky with half-hour turns each on the nights of Nov. 14th and 15th, and about 30 members of the University were present. The number of meteors seen in the half-hours between 3 o'clock and 6.30 on the morning of Nov. 15th, civil time, were 2, 4, 3, 8, 9, 18, and 3, the maximum number of 18 being seen between 5.30 and 6 o'clock. The report from Durham states that the sky for the purposes of observation was divided into 6 segments, meeting at the radiant and passing through certain pairs of stars. Each observer was to count the number of meteors appearing per minute in his own segment. The display was so meagre that little interest attached to the observations. Mr. Sampson gave details of observations made by himself and Mr. Carpenter, and a Table analyzing the number that appeared in successive quarters of an hour from $15^h\ 15^m$ to $18^h\ 15^m$, which were 2, 1, 5, 2, 3, 3, 2, 4, 2, 2, 2, 4. Lastly a communication has been received from the Royal Observatory, Edinburgh. Considerable attention was given to the determination of the radiant and the position was deduced from observations of pairs of Leonids, each pair being made first of all to give a single radiant and then the different radiants to give a mean. These were all the communications that had been received with reference to the observations of the Leonids.

Mr. Lindemann. I was at Wiesbaden on the 14th, and tried to make observations there. But on that night there was bad weather and on the 15th I thought it wise to ask the Director of the Observatory at Heidelberg if he had seen anything, and he informed me that he had seen 140 on the previous night. I requested the police and the porter at the hotel if anything occurred to wake me, and to be sure to wake me at 4 o'clock. I observed from 4 to 6 o'clock in the morning. The sky was not cloudy and one could have observed very well, but I did not see a single meteor. On the 16th I waited until 3 o'clock in the afternoon, when San Francisco might have been able to give some information, and I telegraphed to Kiel and asked whether they had seen anything and whether anything had been seen in Asia or America. I had a long telegram in which they said that the sky was pretty good and that they had seen about 200 Leonids in Kiel, but they had no news from Asia or America. They had taken trouble in Germany to observe this shower, and a pamphlet had been published and distributed very widely all over South Germany asking people to observe the shower.

The Chairman. Mr. Lindemann has given us some valuable information on the subject. I watched on the night of the 14th, beginning at midnight, until nearly 2 o'clock in the morning of the 15th and I saw 5 meteors. I should not like to say they were all Leonids. The next night there was a fog that you might have cut with a butter-knife, and on the night of the 16th and morning of the 17th I did not see a single meteor. I must confess that after the researches of Dr. Johnstone Stoney and Dr. Downing I was not particularly hopeful about the matter.

A Fellow. Can the falling off in numbers be accounted for?

The Chairman. I am afraid that question is unanswerable. That the stream has been perturbed by the action of the large planets, is, I think, common knowledge, but there is something beyond that which we do not know. We shall have to wait until 1900 before any answer can be given, even if it can be given then.

Mr. Newall said that a paper had been received from Mr. J. E. Keeler on new nebulæ discovered photographically with the Crossley Reflector of the Lick Observatory.

The following papers were announced and partly read:—

E. W. Brown. "Note on the Values of the Coefficients of the Terms of the Third Order in the Lunar Theory."

H. H. Turner. "Observations of the Leonids at the University Observatory, Oxford."

F. A. Bellamy. "On the Proper Motions of Berlin B. 5072 and 5073."

J. E. Keeler. "New Nebulæ discovered photographically with the Crossley Reflector of the Lick Observatory."

Durham Observatory. "Observations of the Leonids."

W. Ellis. "On the Relation between Magnetic Disturbance and the Period of Solar-spot Frequency."

Rev. T. E. R. Phillips. "The Extra-Equatorial Currents of Jupiter in 1899."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1898, deduced from Photographs taken at the Royal Observatory, Greenwich, at Dehra Dûn (India), and in Mauritius."

Radcliffe Observatory, Oxford. "Observations of the Leonids of 1899, November 14 and 15."

G. Clark. "Results of Observations of the Leonid Meteors of 1899, as seen from the Royal Observatory, Edinburgh."

Royal Observatory, Greenwich. "Observations of the Leonid Meteors of 1899."

The following gentlemen were elected Fellows of the Society:—

H. C. Plummer, Owens College, Manchester.
Clement Jennings Taylor, Port Elizabeth, Cape Colony.

The following Candidate was proposed for election as a Fellow of the Society:—

William Henry Robinson, Offendene, Walsall, Staffordshire (proposed by Sir R. S. Ball).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The first ordinary Meeting of the tenth Session of the Association was held at Sion College on Wednesday, November 19, the President, *Mr. W. H. Maw, F.R.A.S.*, in the Chair.

The President, in opening the meeting, said that he had an announcement to make, and he did it with much regret. Since their last meeting they had lost by death their late President, Mr. N. E. Green. Mr. Green, as they all knew, was one of the founders of the Association, and had served it as President, Vice-President, and Director of the Saturn Section. Mr. Green was a representative of the very best type of amateur observers. His training as an artist enabled him to record his observations with exceptional accuracy and skill, and it would be admitted that his planetary drawings had become classic. For fidelity to detail and delicate rendering of colours and shading, these drawings had rarely been equalled, and had certainly never been surpassed.

The Meeting then passed a vote of condolence with Mrs. Green and her family.

The election of fifteen new Members by the Council was confirmed, and the names of eight new Candidates for membership were passed for suspension.

An interim report dealing with the November Leonids was presented by the Director, *Mr. W. F. Denning.*

After alluding to the disappointing character of the display, owing partly to the unfavourable weather, but primarily, no doubt, to the tenuous region of the swarm traversed by the Earth, Mr. Denning said that they been led to the following conclusions:—(1) The duration of the shower was from Nov. 8 to Nov. 18 inclusive; (2) the maximum occurred on Nov. 14 in the hour between 17^h and 18^h, when, however, not more than 25 Leonids were visible to one observer; (3) the radiant-point was at $151\frac{1}{2}° +22°$; (4) the radiant was probably a fixed position according to the paths of several early Leonids, but the evidence they afforded was, perhaps, not conclusive.

Dr. Johnstone Stoney said that while they had to regret the absence of the great display for which they had hoped, it was some consolation that they were in a position to say with confidence what had kept the Earth from this year receiving one of the greater Leonid showers. This was due to the new position into which the meteoric orbit had been shifted since 1866, owing to the quite abnormal amount of the perturbations to which the meteors in the dense part of the stream had been exposed since that year. We were able to follow with accuracy what had happened to the meteors which occupied one special position in the great procession—namely, that portion of the stream through which the Earth passed in 1866. The late Prof. Adams succeeded in determining what was at that time the form, the size, and the position of the immense elliptic orbit round the Sun along which these meteors travelled. Fortunately, the alterations which Adams's orbit had undergone within the last thirty-three years had been accurately followed by the skilled computers of the *Nautical Almanac*, and it had thus been ascertained that the changes of the orbit due to perturbations had been of unusual amount during this revolution, chiefly because the great planet Saturn approached close to the meteors upon their outward journey, and Jupiter while they were returning. In 1866 the great elliptic orbit accurately intersected the Earth's orbit at one point. The ellipse has since shifted its position to the extent of 1,300,000 miles, so that at six o'clock on the morning of Nov. 16, when the Earth came closest to the meteoric orbit, the nearest point on that orbit lay inside the Earth's orbit at the above distance from the Earth—that is, it was more than five times as far off as the Moon. What the non-appearance of a great shower this year had established was that, wide as was the ortho-stream, it had not extended so far beyond the point whose position was known as 1,300,000 miles.

The President asked Dr. Downing to give the meeting what information he possessed with reference to the shower next year.

Dr. Downing said that unfortunately the conditions would be still worse next year than those which prevailed on Nov. 15 just passed. The situation of the dense part of the stream which would reach the node next November, and whose position they had been able to calculate, instead of being 1¼ million miles, would be about 1¾ million miles nearer the Sun than the Earth would be at the time, so that he was afraid that there was less chance than on the present occasion of there being any large display.

Mr. G. J. Newbegin said he had a feeling that we had lost the Leonids. It was probable that the orbit had been contracted under the influence of the major planets Saturn and Jupiter, and in that case the Earth would in future miss the Leonid stream altogether.

Captain Noble thought it was to be regretted that the researches of Dr. Downing and Dr. Stoney had not been made a little more public than they were. Up to the last moment newspapers and magazines had been holding out hopes of something amazing in the shape of a display.

The Rev. G. Castleden called attention to the very remarkable sunsets seen recently, brilliant golden crimson with a green sky, recalling the splendid sunsets in 1883, which were attributed to the dust from the eruption of Krakatao, and he wondered whether the dust scattered by the Leonids and Andromedes could have had any connection with the phenomena in question.

The President asked Dr. Stoney as to the prospects of being able to photograph the Leonid stream next year. He understood that Dr. Roberts had attempted it this year, but without success.

Dr. Downing said that the present year was the best chance Dr. Roberts had had, and that it was quite hopeless next year.

Mr. Maunder read a *précis* of a paper on "Jupiter's Equatorial Current," by *M. Comas Solà.* The writer attributed the rapid changes in speed of the equatorial spots to causes outside the planet, and concluded that there must be one or, perhaps, several small satellites very close to the primary, and with an angular velocity a little greater than that of the planet.

Mr. C. T. Whitmell read two papers entitled respectively, "Tides on Jupiter" and "The Shadows of Jupiter's Satellites." The writer concluded that the planet's high temperature was probably the main cause of the remarkable surface-changes, but suggested that the tidal action of the satellites might be an operative cause.

Mr. Crommelin said it was rather a large order to require a new and invisible satellite to explain the tides on Jupiter. M. Solá must have forgotten the extreme weakness of the gravitational force. It would require many thousands of unseen satellites to account for the phenomena.

A number of lantern-slides of nebulæ, taken with the Crossley reflector at Lick, were then thrown on the screen.

The President said that he was glad to announce that the Rev. J. M. Bacon and Miss Bacon were present. They had all been heartily glad when they received the good news that Mr. and Miss Bacon had accomplished their adventurous journey without any further mishap than that which befell them. Mr. Bacon had very kindly promised to give them an account of that journey.

Mr. Bacon then proceeded to give a graphic and interesting account of his ascent, illustrated by a number of lantern-slide views. Although the observers were well above the clouds in a beautifully clear atmosphere, the number of meteors seen was disappointingly small, and few were of any brilliance. Mr. Bacon concluded by referring to the next year's solar eclipse, and his proposed expedition to America to observe it. He said that it would be necessary of any members who proposed to join it to make immediate arrangements, on account of the enormous traffic expected in connection with the Paris Exhibition.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, December 20, at the Institution of Civil Engineers, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. Baldwin Latham, M.Inst.C.E., read a paper on "The Climatic Conditions necessary for the Propagation and Spread of Plague." The bubonic plague is primarily due to a specific organism or microbe of infinitesimal size—so small that probably 250 millions of them would be required to cover a square inch of surface. Plague is infectious and contagious, and is greatly influenced by pestilential emanations from polluted and water-logged soils. The author gives accounts of various outbreaks of plague in this and other countries, including the great plague of London in 1665, when 7165 deaths were recorded in one week in September. Plague is undoubtedly a disease of the poor, and attacks most readily those living on a low diet. The conditions which are conducive to the spread of plague are identical with those which give rise to the escape of malaria from the ground. That the ground itself exercises an enormous influence upon plague is shown by the fact that in all the epidemics persons living on the ground-floors suffer to a much greater extent than those who live in the higher storeys of the houses. Mr. Latham said that there cannot but be a doubt that the conditions which ordinarily produce evaporation from water or land surfaces are identical with those which produce exhalations from the ground; and these exhalations consist largely of vapour of water carrying matters injurious to health with them. Mr. Latham has discussed the meteorological observations (including the temperature of the soil at the depth of 9, 20, 60, and 132 inches) made at the Colaba Observatory, Bombay, and has compared them with the number of deaths from plague during the recent epidemics in Bombay. He says that if the temperature of the air increases beyond the temperature of the ground, so that its dew-point is above the temperature of the ground, condensation takes place instead of evaporation. To this increased high temperature may be due the sudden stoppage of plague after a certain high temperature has been reached; which, by raising the temperature of the dew-point, stops all exhalation from the ground, and may cause condensation to take place instead of evaporation. So also a sudden fall of temperature causes plague to arise; for a fall of temperature means that the temperature of the dew-point must fall, and the tensional difference between a low dew-point and a high ground temperature would at once lead to exhalations escaping in large quantities from the ground, and so lead to the liberation of the plague bacillus from the ground, accompanied by the exhalations necessary for its development.

Dr. R. H. Scott, F.R.S., communicated a note on a remarkable Dust Haze which was experienced at Teneriffe, Canary Islands, on February 16 to 19, 1898. The haze during this period was exceptionally dense, so much so that a steamer was two days and three nights on a voyage from Teneriffe to Las Palmas, a distance she usually covered in five hours; while the 'Tintagel Castle,' of the Donald Currie Line, was delayed for thirty hours, and the 'Roslin Castle,' homeward bound, had the dust so thick that for 900 miles the Sun and stars were obscured and the ship was delayed two days.

The Quadrantid Meteor-Shower.

The year opens with the epoch of the Quadrantids, the maximum of which meteor-shower occurs between January 1 and 3, though some members of the group are generally seen at the end of the preceding month. It will therefore be well to draw attention to the expected recurrence of this shower, especially as its returns in some recent years have been well marked. This may, perhaps, best be done by summarizing its history and, in passing, remarking on the characteristics of the meteors.

On 1835, Jan. 1, 16^h until dawn, an extraordinary appearance of shooting-stars was seen by Wartmann at Mornex, near Geneva.

1838, Jan. 1, 15^h.—A similar phenomenon was observed at Planchettes and Chaux-de-Fonds.

1839, Jan. 2.—Meteors were recorded by Bravais at Bossekop, Lapland.

1840, Jan. 2.—Shooting-stars were seen at Ghent.—Prof. DUPREZ.

"Among the remarkable nights on which shooting-stars appear periodically in great numbers I believe I am the first to draw to the attention of meteor observers that of January 2nd or 3rd, and for seven years the event has justified the prediction. For example, when, during that winter's night, the sky has been clear, as in 1835, 1838, and 1840, people have seen from several parts of Europe, especially from midnight to dawn, an unusual phenomenon of very brilliant meteors. This epoch will, then, take rank in the records of meteor observation with that of August 10th."

Thus wrote Wartmann at Geneva in 1841. As he thought, he was the first to distinguish the period in question as one favourable for the appearance of shooting-stars.

On 1843, Dec. 28, 29, many meteors were seen by Cooper at Nice; these were possibly connected with the January shower.

1848, Jan. 2, 18^h.—Many falling-stars.—E. HEIS, Aix-la-Chapelle.

Jan. 2, 3.—An extraordinary number.—COLLA, Parma.

Jan. 4, 6^h.—Several small falling-stars.—E. J. LOWE, Nottingham.

1849, Jan. 1.—"Many shooting-stars in evening and morning. A fine one at 11^h 46^m, of which the duration was 5 secs."—E. HEIS.

1857, Jan. 2, 14^h–$17^h 30^m$.—A number of meteors were seen by an assistant at Wrottesley Observatory, Wolverhampton.

1859, Jan. 2, 10^h–11^h.—A great number of meteors, about one per minute."—F. MORTON, Wrottesley Observatory.

In 1861 the fact of the recurrence of the meteor-showers at the beginning of the year was again brought forward, this time by Quetelet, who specified the date—Jan. 2.

Henceforward observations became more numerous, and in 1863 the radiant-point was first determined by Stillman Masters in the United States, who recorded "eight very luminous excursions" at daybreak on Jan. 2. He fixed it at $248° +47°$.

The following year appears to have brought round a maximum of the shower.

1864, Jan. 2, 7^h 30^m–8^h 30^m.—"Very many meteors."—E. J. LOWE, Beeston.

10^h–12^h.—50 shooting-stars (26 Quadrantids), white or yellow (3, 21, 17, 3, 6 of 1st magnitude, &c.: 1, 16, 14, 3, 4 $< \frac{1}{2}$, 1, $1\frac{1}{2}$, 2, and 3 secs. in duration). These were seen in all parts of the sky. —Prof. A. S. HERSCHEL, Hawkhurst, Kent.

10^h–13^h.—50 shooting-stars, all bright white (4, 14, 23, 27 of 1st, 2nd, 3rd, and 4th magnitudes respectively; 3, 13, 6 $< \frac{1}{2}$, 1, and 2 secs. in duration, remainder not noted). The meteors were in all parts of the sky, six left trains, and one, twice as bright as Jupiter, burst into sparks.—R. P. Greg, Manchester.

11^h–$11^h\ 30^m$.—Ten shooting-stars, two very brilliant.—H. Weightman, Bolton.

12^h–13^h or 14^h.—Shooting-stars fell at the rate of one per minute.—W. H. Wood, Weston-Super-Mare. Prof. Herschel fixed the radiant-point at *c* Quadrantis.

1867, Jan. 2, 11^h–$11^h\ 45^m$.—"Several meteors."—R. P. Greg.

1870.—17 Quadrantids were seen by Captain (now Lieut.-Colonel) Tupman in the Mediterranean.

1872, Jan. 2, 10^h–$11^h\ 30^m$.—29 meteors, "white, generally bright, and left faint streaks."—H. W. Jackson, Tooting.

$10^h\ 12^m$–$11^h\ 17^m$.—15 meteors, "only 4 less than 1st magnitude, mostly bluish, but some yellowish-white."—Assistants at Greenwich Observatory.

$10^h\ 15^m$–$11^h\ 15^m$.—15 or 16 meteors mapped; "flight generally 1–$1\frac{1}{2}$ sec.; slow, trains, mostly blue, white, or yellow."—W. H. Wood, Birmingham.

$10^h\ 30^m$–12^h.—16 shooting-stars, "6 as bright or brighter than 1st magnitude; brightest white, and those of lesser magnitudes of yellow colour" ($\frac{1}{2}$-hourly rate, 3, 5, 8).—Prof. A. S. Herschel, London.

$10^h\ 45^m$–12^h.—9 meteors (6 Quadrantids, 3 = 1st magnitude). "All but one, which appeared red, were white or bluish, not swift in their motion, and two of the brightest left a short streak of light upon their course."—T. Crumplen, London.

$11^h\ 20^m$–12^h.—14 meteors, principally 1st and 2nd magnitude, "white, long, moderately slow; about half left a slight persistent streak of light on the whole or a part of their course."—Two observers at Hawkhurst, Kent.

1873, Jan. 1, 13^h–14^m.—Several bright meteors were seen at Wrexham.

17^h–19^h.—31 meteors (27 Quadrantids).—T. W. Backhouse, Sunderland.

1878, Jan. 1, 16^h–$16^h\ 30^m$.—17 Quadrantids, 2 = Jupiter or Sirius, 5 = 1st magnitude, 6 = 2nd, and others = 3rd or 4th; no streaks, short and quick.—Prof. A. S. Herschel, Hawkhurst, Kent.

1879, evenings of Jan. 1 and 2 and morning of Jan. 2.—8 or 10 fine shooting-stars per hour in each watch.—Prof. A. S. Herschel, Newcastle.

Jan. 1, $18^h\ 15^m$–$18^h\ 35^m$.—14 Quadrantids, no other meteors.—W. F. Denning, Bristol.

1880, Jan. 2, 6^h–$9^h\ 30^m$.—25 meteors (19 Quadrantids), "rather slow with spark trains, average length of path $15\frac{1}{2}°$."—W. F. Denning.

6^h–10^h ($3\frac{1}{4}^h$).—66 meteors (48 Quadrantids), "some streaks, some trained."—H. Corder, Writtle.

7^h–8^h.—6 Quadrantids, "quite bright, long, and generally rather slow."—E. F. Sawyer, Cambridgeport (Mass.), U.S.A.

1886, Dec. 28: 2 Quadrantids. Dec. 29: 3 Quadrantids. Dec. 31–1887, Jan. 4: 8 Quadrantids.—W. F. Denning.

1888, Jan. 2, $10^h\ 3^m$–$13^h\ 37^m$ ($2^h\ 22^m$): 33 meteors. $18^h\ 5^m$–$19^h\ 21^m$ ($1^h\ 8^m$): 16 meteors; 6 Quadrantids in the $1\frac{1}{2}$ min. preceding $13^h\ 6\frac{3}{4}^m$.—T. W. Backhouse.

$10^h\ 43^m$–12^h.—6 meteors (5 Quadrantids), "long paths, moderate speed."—W. F. Denning.

1892, Jan. 1–6.—39 Quadrantids.—H. Corder, Bridgwater.

Jan. 1–2.—25 Quadrantids, "rather swift, streaks."—Dr. Booth, Leeds.

1893, Jan. 1, 12^h–14^h.—30 Quadrantids.—W. H. Milligan, Belfast.

1894, Jan. 1.—During $3\frac{1}{2}^h$ 19 meteors were recorded (7 Quadrantids).

Jan. 2, $6^h\ 55^m$–$7^h\ 35^m$, hourly number very high (40 for a short time), then dropped to 4 or 5, and rose to 8 between 11^h and 12^h (13 Quadrantids).—H. Corder.

In 1895 and 1896 observations were few.

1897, Jan. 1, $4^h\ 54^m$–$18^h\ 49^m$ (during $4^h\ 15^m$ between these times).—97 meteors, of which 25 were of magnitude 0, 5 = Sirius, and 2 = Jupiter; nearly all Quadrantids (83 or 86). Maximum after 16^h.—T. W. Backhouse.

$6^h\ 30^m$–$16^h\ 40^m$ (about 6^h clear, mostly after $11^h\ 20^m$).—130 meteors (109 Quadrantids); hourly number, 30–34.—H. Corder.

No observations of the Quadrantids were possible in 1898, and apparently but few were made in 1899.

It has been assumed that the returns of the Quadrantids are governed by a 13-year period, but the amount of evidence in favour of this theory does not seem to be satisfactory.

The Quadrantids form a class of meteor very unlike the Geminid type. Their paths are long and their motion rather slow; the meteors are bright with faint streaks.

Mr. Denning's list of 32 radiants (No. clxxiii. "General Catalogue") with a mean at $230^\circ{\cdot}6 + 51^\circ{\cdot}1$ contains but one instance of a doubly-observed meteor. In this case the speed was 13 miles per second.

There is no reason for believing the radiant-point to be a shifting one. Meteors from nearly the same centre have been recorded from the end of January to the middle of May and from the last half of July until October.

W. E. Besley.

75, The Chase, Clapham Common.

Southern Double Stars.

When we say that *The Observatory* has always had its eye on that branch of our science which is devoted to double stars, we make no claim to expressing a fact in the most classical or elegant language; but we venture to think that the expression discovers the position as no other could. We have urged observers to work in certain directions, we have applauded good work always, and invariably pointed out faults; indeed, it may have appeared that at times we have been hypercritical, but this was in appearance only, as the eye has throughout been most friendly disposed. As far back as 1878, when the magazine was just feeling its way under the care of the present Astronomer Royal, a fine series of papers on double stars appeared. These were from the pen of Dr. Doberck, and, in connection with the monthly lists of suitable stars for observation, were calculated to create real interest in the work. After this we come to the appeal by Gledhill for more workers, while in recent years we have been able to give a short historical sketch of the work. Beyond this, the point we wish to emphasize at the moment is that, in season and out of season, we have never ceased to draw attention to the fact that the southern skies were too much neglected. Double-star work in the southern hemisphere has always been of a most desultory kind, and it has always given us real pleasure to notice that the Sydney Observatory, aided by occasional efforts of Mr. Tebbutt, has prevented utter collapse. Recently, Prof. Glasenapp, the Lick observers, and others have been busy with the pairs more near the Equator. Quite recently we noticed the excellent work of Dr. See with the 24-inch Lowell refractor, work of which both Dr. See and Mr. Lowell may well be proud. It gives us measures of old pairs and furnishes many new and interesting ones, thereby creating and reviving interest in Southern Doubles, but at the same time tending by these very additions to retard progress. Something was urgently needful to place double-star work in the south on a proper footing, to entice astronomers to its pursuit by enabling both the amateur and professional to plan his work with the greatest advantage, something which would enable the observers to conserve their energy for use on certain systems, and provide at a glance the knowledge that their work was of real value.

This something was a 'Catalogue of Reference,' which one had but to consult to gain a knowledge of what was required and an idea of his ability to help. That an embryo catalogue existed in manuscript was known to us, but that it should be brought up to date and published we never dared to hope. "It is the unexpected that happens." This very Catalogue we have now before us dressed in yellow, and rejoicing in the title of 'Annals of the Royal Observatory, Cape of Good Hope,' vol. ii. part ii. "Reference Catalogue of Southern Double Stars."

Such a catalogue, beyond its usefulness as a double-star catalogue simply, is of value when the usual star catalogues are in formation, and the author of the present work goes even further. He says: "The programme adopted by the *Comité International de la Carte du Ciel* requires the selection of ten stars on each plate for meridian observation as points of reference for the remainder. It is obvious that these standard stars should not be double; if the components are not shown separately on the plates, it is inadmissible to assume that the photographic light-centre of gravity, so to speak, agrees with that resulting from eye-observation. This objection applies even to optical or fixed pairs. In the case of binary systems, even if both stars are shown on the plates, it is clearly advisable to avoid the complications that arise through their introduction. It will be evident that the scheme of this catalogue excludes no double star that would appear single on a good plate; hence it may be found useful when selecting reference stars by showing what stars to avoid." How far this holds in practice we confess to have but a vague notion, not being *au fait* with astrographic methods. Consideration of the measurement of plates carries with it the idea of zone catalogues, and one imagines a number of the doubles would be marked therein, and we cannot conceive of any large number being left over. It is a point, however, which some of our astrographic readers might discuss.

Great credit is due to Dr. Gill for not only perceiving that "the time is opportune for the appearance of a Reference Catalogue," but for actually securing such appearance. As regards its author, we cannot do better than quote Dr. Gill's introduction: "Mr. R. T. A. Innes, the author of the present work, joined the Staff of the Cape Observatory in 1896 as Secretary, Librarian, and Accountant. It formed no part of his official duties either to engage in astronomical observing or to contribute in any way to the publications of the Cape Observatory. But previous to his arrival at the Cape he had devoted himself to practical astronomy, and, with comparatively feeble means, had discovered about forty new double stars, and published their estimated distances and position-angles. He had also made some progress in the preparation of a catalogue of reference to the known double stars of the Southern Hemisphere. This catalogue he has not only completed within the last two years in the intervals of his leisure time, but he has discovered upwards of 280 new double stars with the 7-inch equatorial, and contributed a large amount of valuable work to the revision of the *Cape Photographic Durchmusterung*."

And now let us see what Mr. Innes thinks are the possibilities opened out by a careful study of double stars. "The study of double stars will always be interesting from the Newtonian point of view, and in the case of the brighter pairs, the spectroscopic determination of motion will lead to a knowledge of the true position in space or of the absolute dimensions of their orbits, and hence their parallaxes. The irregularities in their orbital motions (already ascertained in a few cases) indicate the presence of disturbing bodies. From the astrophysical point of view it is hoped that much may be learned from their study. When we are able to arrange the elements of the orbits of say 1000 pairs in tabular form, with their spectroscopic history, both of constitution and of radial speed, including for each the proper motion and its solar component, we shall be in a position to attack questions relating to the construction of the sidereal heavens, which are to-day beyond the realms of reasonable speculation. In spite of neglect, the southern systems already exceed in importance those of the north. Among them may be quoted: Sirius, α Centauri, γ Virginis, γ Lupi, γ Centauri, γ Circini, γ Sextantis, ζ Aquarii, o_2 Eridani, α Crucis, β Muscæ, 4 Aquarii, π Lupi, ξ Scorpii, β Phœnicis, ζ Sagittarii, &c.

"It is true that α Crucis is not a binary. It is perhaps all the more remarkable, being one of the brightest stars in the sky, and shown by photography to be surrounded by a cluster or rather by rings of very faint stars, in the centre of which the two large stars are, as far as can be ascertained, absolutely stationary."

Some frailties inseparable from us poor humans sometimes turn out real advantages, and Mr. Innes is to be congratulated on possessing so robust a belief that "the southern systems *already* exceed in importance those of the north." It is a good creed for Mr. Innes, and it is devoutly to be hoped that so sound a working hypothesis, in a milder form, will take root in every southern observer. This does not prevent us Northerns hugging the belief that *already* is a little too previous. "Are not Abana and Pharpar, rivers of Damascus, better than all the waters of Israel?"

Indeed, there is a too general misconception of the state of our knowledge in respect to double stars. So far as things are at present, the great bulk of knowledge naturally rests with the northern systems, they having been under longer and more careful observation. A few years hence the Burnham stars will undoubtedly materially increase our grasp of the subject, but even here the northern systems will far exceed those of the south.

Double stars may be interesting for various reasons:—

(1) The fact of a star being binary is the chief point, and if the period is fairly well known then its interest is increased.

(2) Stars known to be binaries, but whose period we can at present merely make a guess at, and stars physically connected, come next in interest, which interest varies according to the special peculiarities of each pair, such as contrast of colour or magnitude, separation, proper motion, and spectroscopic motion.

(3) Stars which cannot fairly be included in either of the above but have some special peculiarity.

(4) The ordinary double.

In the following list will be found those stars, with their periods, which may be included in class (1). β 883 and 95 Ceti are of very short period, possibly under 10 years, but not yet determined.

Northern Hemisphere.		Southern Hemisphere.	
β 883	?	95 Ceti	?
κ Pegasi	11·4		
δ Equulei	11·5	β 395	16
β 524	22	ζ Sagittarii	19
85 Pegasi	24	9 Argûs	22
42 Comæ	26		
β Delphini	28		
Σ 3121	34	β 416	33
ζ Herculis	35		
β 612	36		
τ Cygni	37		
η Cor. Bor.	42		
μ' Herculis	45		
OΣ 269	48	Σ 2173	46
OΣ 298	52	Sirius	52
γ^2 Andromedæ	54		
99 Herculis	58		
ξ Ursæ Maj.	60		
ζ Cancri	60		
γ Cor. Bor.	73		
OΣ 285	77		
OΣ 234	77		
OΣ 235	80		
OΣ 400	81	α Centauri	81
70 Ophiuchi	88	γ Lupi	83
Σ 228	89	γ Centauri	88
λ Cygni	93	8 Sextantis	92
OΣ 224	96		
φ Ursæ Maj.	97		
Σ 3062	105	ξ Scorpii	105
ω Leonis	115		
ξ Boötis	128	4 Aquarii	129
36 Andromedæ	140	γ Cor. Aust.	153
25 Can. Ven.	184		
η Cassiopeiæ	196	γ Virginis	194
μ^2 Boötis	220		

These are by no means all the pairs for which orbits have been computed. Roughly there are 50 more, and these would be proportioned North and South as 4 to 1. No useful purpose can be served by pursuing the subject further, especially as the list given by Mr. Innes by no means represents his case to its fullest advantage.

The Catalogue contains, in addition to many rather wide pairs, worthy of inclusion from some point of view, every known southern double star which falls within the following limits of distance :—

Mag. of Primary.	Limiting dist. of comes.	Mag. of Primary.	Limiting dist. of comes.
	″		″
1	30	6	7
2	25	7	5
3	20	8	3
4	15	9	1
5	10		

in all 2140 stars, of which

21 per cent are separated less than 1″.
24 ,, ,, ,, between 1″ and 2″.
55 ,, ,, ,, over 2″

Hence Mr. Innes infers that the pairs under 2″ probably exceed in number those between 2″ and 7″, remembering that the latter class can be easily seen, and that with few exceptions all pairs under 2″ distance require special care to detect. This is too important to accept offhand, and just now there is insufficient time to go thoroughly into the question. So many points are involved, and it is unsafe to found such an argument on existing catalogues which were made for the most part for special purposes. Judging from our own experience, Prof. Burnham must have seen numbers of doubles 5″ or 7″ apart which he did not catalogue. The following table shows the special characteristics :—

W. Herschel	29	stars under 2″	94	between 2″–8″.
W. Struve	120	,, ,,	340	,, 2″–8″.
O. Struve	222	,, ,,	107	,, 2″–8″.
Burnham	723	,, ,,	346	,, 2″–6″.

The stars in the catalogue are arranged in order of R.A. (1900), with their declinations, magnitudes, and measures, supplemented, where required, with copious notes. In a few cases only have all the measures of a pair been quoted, a selection sufficient to show the nature of the pair. Considering the unsatisfactory nature of many of the measures, this is a wise course provided the latest measures are given. In a general way we believe this has been done. There are many other points, such as magnitudes, nomenclature, colours, and personal equation, which Mr. Innes has brought forward in a very interesting way. The question of magnitudes alone must have been an enormous piece of work. Great care has been taken to name each star after its discoverer, perhaps too much care. For instance a star appears as Lalande 12, and at the end of the measures (commencing with Σ) we learn that it is Σ 218. How much better to call it straight away Σ 218! What a pity it is that any observer ever thought of personal equation in connection with double-star work. If the computer wants it, let him find it; but do let us have original measures. Here is the state of mind Mr. Innes is left in through it :—" Many astronomers have *adopted* corrections to their observed angles and distances. Some, at least, of these corrections are of doubtful utility, but in transcribing the results, *they have sometimes been used and sometimes omitted.*" The italics are ours.

At the end of the Catalogue are indexes and more recent measures, with a most valuable list of works and papers on double stars, each work accompanied by concise notes as to its contents, forming a worthy conclusion to an eminently useful, interesting, and practical work; and may it, as we believe it will, give an impetus to observers and investigators, thereby, in some measure, rewarding Mr. Innes for his labours. T. Lewis.

Variation of Personal Equation with Stellar Magnitude.

In comparing the results of observations made with the heliometer and transit-circle it has been found that certain differences of Right Ascensions are found to increase in amount in order of star-magnitude—in other words, the Right Ascensions of the fainter stars from observations with the transit-circle are found to be systematically large compared with the brighter stars; the fainter the star the greater is the error of Right Ascension as given by a transit-circle observation. As early as 1831 certain discordances in heliometer observations attracted Bessel's attention to such a possibility, but it was not till 1878 that the question forced itself into prominence. During the favourable opposition of Mars in the previous year Dr. Gill was making observations with the heliometer to determine the parallax of the Sun, and these observations afforded such clear evidence of this discordance that he appealed to the authorities of several observatories who were making a special series of observations of stars for him to investigate the matter.

Consequently investigation of the subject was made during the ensuing years by Bakhuyzen at Leiden, Boss at Albany, Becker and Küstner at Berlin, Gill at the Cape, and by the Astronomer Royal at Greenwich. The method generally adopted was to place in front of the object-glass screens of fine wire gauze, which partially obscured the star's light, and to make observations of the star in course of transit over half the wires with its natural or full light, and over the other half with its light artificially reduced by the insertion of the screen before the objective. The excess of the time of transit with screened object-glass over the time with unscreened object-glass divided by the estimated reduction of light in terms of star-magnitude constituted the amount of error for one magnitude. The chief objections to this method seem to be the difficulty of determining the absolute value of the scale of the screen and an injurious effect that the interposition of the screens might have on the star's image; and beyond these, the tacit assumption made in the reduction that the error is constant over the whole range of star-magnitudes. A few of the results given below will show the possible amount of variation among different observers, and also give a criterion of the general character of the error. The values for the Greenwich observers have been obtained by taking the mean (1·75 magnitude) of the photometric values of the screens given by the Astronomer Royal in the paper on the subject (Mon. Not. vol. li. p. 455); the sign + indicates that the fainter star is observed too late.

Observer.	Value for one mag.	Observer.	Value for one mag.
	s		s
Becker (Berlin)…………	+0·0070	Thackeray (Greenwich).	+0·0265
Boss (Albany) …………	+0·0140	Lewis (Greenwich) ……	+0·0146
Küstner (Berlin) ………	+0·0029	Hollis (Greenwich) ……	+0·0196
Downing (Greenwich) …	+0·0145	Graham (Cambridge) …	+0·0200

In the 'Annals of the Cape Observatory,' vol. vii. pp. 416–426 and 704–716, Dr. Auwers collected and discussed all available data on this question with reference to the observations of the stars made for Dr. Gill by various observatories in connection with his determination of the solar parallax from observations of the planets Victoria, Sappho, and Iris. As a result of the discussion the following table was deduced, exhibiting the observed and theoretical values for the mean observer at the observatories of Berlin, Leiden, Cape, Leipzig, Greenwich, and Cincinnati for one magnitude:—

Observatory.	Variation of deviation from Berlin system for one mag.			Empirical correction.
	Iris series.	Victoria series.	Sappho series.	
	s	s	s	s
Berlin…………………………	0·0000	0·0000	0·0000	0·0000
Leiden ……………………	−0·0007	+0·0139	−0·0030	−0·0007
Cape $\frac{1}{2}$(M+P) e.c. ………	+0·0012	……	……	−0·0128
Cape $\frac{1}{3}$(M+P+C) chr. …	……	+0·0221	……	−0·0127
Cape $\frac{1}{2}$(M+P) chr. ………	……	……	+0·0229	−0·0161
Leipzig ……………………	+0·0135	+0·0110	+0·0105	−0·0087
Greenwich…………………	+0·0226	+0·0202	+0·0238	−0·0155
Cincinnati…………………	……	+0·0072	+0·0162	−0·0131

Prof. Newcomb, in his 'Catalogue of Fundamental Stars for 1875 and 1900, reduced to an Absolute System,' pp. 168–185, has also discussed this subject at some length. From a comparison of various catalogues he arrives at the conclusion that there is no well-marked difference between the general amount of this error for one magnitude in eye-and-ear and chronographic observations, as will be apparent from the following table which he gives. The + sign indicates that the fainter star is observed too late :—

Observer.	Eye-and-ear.	Observer.	Chronograph.
	s		s
Romberg	+0·0123	Pulkova observers, 1865 .	+0·0124
Pulkova observers, 1845 .	+0·0067	Küstner (Berlin)	+0·0032
Greenwich observers, 1845–53................	+0·0074	Greenwich observers, 1854–86................	+0·0087
Cape observers, 1879–86.	+0·0118	Boss (Albany)	+0·0126
Paris observers, 1860–80.	+0·0075	Washington observers, 1862–65...............	+0·0048
		Washington observers, 1866–68................	+0·0091
Mean	+0·0091	Mean	+0·0085

In the introduction to the Cape Catalogue, 1890, p. xxxii, Dr. Gill, from a comparison of various catalogues, shows there is no sensible systematic difference depending on magnitude in the case of the declinations, nor, indeed, can any be traced in the case of the Right Ascensions; but this, as Dr. Gill points out, is not because such differences do not exist, but that in most catalogues observations depend on many observers, and that in the main the average magnitude personality will be fairly constant.

Prof. Turner, in *Monthly Notices*, lx. 1, pp. 3–12, has contributed the latest paper on this subject, and rightly concludes, we think, that the method he has adopted, viz. the use of photographic measures, is much the better method for determining this insidious error. From a comparison of the Oxford photographic plates with the Cambridge Catalogue, 1875, for which comparison more than 600 plates have been already measured, Prof. Turner has deduced a very valuable determination of this error from magnitudes 5·0 to 9·5. Unfortunately, the stars brighter than 5·0 in the region under discussion are not sufficiently numerous to give a reliable determination. It seems as likely as not that for stars down to mag. 5·0 there may be no very appreciable variation of magnitude personality. For stars from magnitude 5·0 to 8·0 Prof. Turner finds that this quantity can be well reproduced in the form of a correction,

$$\text{constant } -0^{\mathrm{s}}{\cdot}023 \times \text{magnitude},$$

and from 8·0 magnitude, where the gradient sensibly changes downwards,

$$\text{constant } -0^{\mathrm{s}}{\cdot}100 \times \text{magnitude}.$$

The observed quantities are as follows :—

Comparison between Oxford Photographic Measures and Cambridge Transit-Circle Observations.

(The sign + indicates that the Cambridge R.A. or Dec. is too small.)

Limits of Magnitude.	No. of Stars.	R.A.	Dec.
		s	''
1·0 to 2·9	4	+0·118	−0·12
3·0 „ 3·9	7	+0·113	+0·18
4·0 „ 4·9	9	+0·180	−0·39
5·0 „ 5·4	38	+0·131	−0·21
5·5 „ 5·9	30	+0·114	−0·30
6·0 „ 6·4	88	+0·104	−0·30
6·5 „ 6·9	147	+0·093	−0·27
7·0 „ 7·4	320	+0·084	−0·33
7·5 „ 7·9	504	+0·071	−0·12
8·0 „ 8·4	572	+0·050	−0·15
8·5 „ 8·9	1226	+0·023	−0·06
9·0 „ 9·4	2001	−0·037	+0·13
9·5 and over	438	−0·066	+0·26

W. G. Thackeray.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comets due in 1900.

Gentlemen,—

I should have been obliged to use the singular number in the heading to this brief communication were it not that I am reminded of the story of the person who, when disappointed by a cloudy sky of seeing an eclipse, innocently asked whether it could not be had over again next day. The last of the periodical comets to which I referred in my letter on the subject at the beginning of this year was the one which was discovered by Mr. Finlay at the Cape of Good Hope on the 26th of September, 1886, and passed its perihelion on the 22nd of the following November. The period was found to be about 6½ years in length, and the comet duly returned in 1893, when it passed its perihelion on the 16th of June. Another return therefore is due about the present time, and probably we may hear of its appearance before or soon after the publication of your January number.

The only other comet expected to return this year is the small one discovered by Prof. Barnard at the Lick Observatory on the 16th of July, 1884, about a month before its perihelion passage. The period was determined to be about 5½ years (5·66 according to Mr. Finlay's calculation, published in *Ast. Nach.* No. 2029, vol. cx.

col. 205), but it escaped observation both in 1889 and 1895; the next return will be due towards the end of 1900.

It should also be mentioned that the comet discovered by Mr. E. Swift in California, on the 20th of November, 1894, was found to be moving in an elliptic orbit, with a period which Dr. Schulhof (*Ast. Nach.* No. 3267, vol. cxxxvii. col. 37) determined to be about 5·8 years, so that a return would be due towards the end of next summer. This is a very interesting body, owing to the circumstance of its probable identity with De Vico's of 1844, and with La Hire's of 1678. Dr. Schulhof thinks it probable that the object seen once by Goldschmidt at Paris, on the 16th of May, 1855, was really the same, and adds:—"Sa distance périhélie est devenue depuis 1855 sensiblement plus grande qu'auparavant, et elle augmentera encore notablement en 1897, époque à laquelle la comète sera plus voisine de Jupiter qu'en 1885. Cette circonstance rendra fort difficile la découverte de la comète à son prochain retour." It would seem that, like Holmes's comet, it is subject to fluctuations of brightness, and that one of these caused its comparative brilliancy and visibility to the naked eye in 1844.

Yours faithfully,
W. T. Lynn.

Blackheath, 1899, Dec. 4.

NOTES.

Comet Notes.—Herr Winther gives in Ast. Nach. 3605 the following formulæ for the corrections required by his ephemeris of Giacobini's Comet (1899 e):—

$$\text{Corr. in R.A.}\ldots\ldots\quad +2^{s}\ +0^{s}\cdot12\ \ (t-\text{Nov. }10)$$
$$\text{Corr. in Decl.}\ \ldots\ldots\quad +0'\cdot4+0'\cdot023\ \ (t-\text{Nov. }10)$$

The comet is probably now beyond the reach of any telescope, but we give an ephemeris for the beginning of January, with the above corrections applied (for Berlin midnight):—

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Jan. 3	19 3 59	22 36	Jan. 11	19 18 44	25 19
7	19 11 19	23 56	15	19 26 13	26 42

Prof. Barnard observed Holmes' Comet (1899 II.) on Oct. 30 and Nov. 4; the correction required by the ephemeris deduced from Zwiers' corrected elements was $+0^{s}\cdot77$ in R.A., $+8''\cdot8$ in Decl. (Ast. Nach. 3604). The comet is probably now too faint for further observation.

Ast. Nach. 3605 contains an article by Dr. E. Strömgren on the cosmogony of comets. He quotes Herr Thraen's researches on the Comet 1886 II., which had a decidedly hyperbolic orbit while in the neighbourhood of the Sun, but which Thraen showed had been moving in a parabola four years before, and earlier still in an ellipse. He gives reasons for supposing that a similar result would be obtained if the planetary perturbations for a long period were computed for other comets for which hyperbolic orbits have been deduced. Hence he considers it probable that no comets have hyperbolic orbits before entering the planetary system, and that comets belong originally to our own system, and do not, as a rule, approach it from other systems. He points out that the planets can influence cometary orbits in two ways, viz.: (1) by their direct action; (2) by their action on the Sun. Some computers appear to have overlooked the latter point. Jupiter and Saturn may sensibly affect a comet's orbit in this manner, even when the comet does not approach them at all closely.

Mr. Frederick H. Seares has investigated in Ast. Nach. 3606–7 the definitive orbit of Comet 1894 IV. (E. Swift), whose identity with De Vico's Comet is probable. The comet was discovered on 1894 Nov. 20, and observed till 1895 Jan. 29. The following are the final elements after applying planetary perturbations:—

Epoch 1894 Dec. 1·0; Osculation 1894 Dec. 10·0 Berlin M.T.

M	8° 22′ 58″·2	±4″·2	
π	345 23 11 ·1	± 4 ·4	1900·0
☊	48 48 23 ·4	±27 ·7	
i	2 57 55 ·8	± 1 ·4	
φ	34 51 37 ·3	± 7 ·1	
μ	605″·9999	±0″·0665	

Mr. Seares proposes to re-examine the question of the identity of this comet with that of De Vico, taking the above definitive elements as his starting-point.

Ast. Nach. 3607 also contains some observations of Tuttle's Comet, 1899 III., made at the Cape Observatory by Mr. Innes. The following are some of the observed positions:—

	G. M. T.				Appt. R.A.			Appt. S. Dec.		
	d	h	m	s	h	m	s	°	′	″
1899 June	26	5	24	17·5....	7	59	30·21	17	3	8·8
July	1	5	16	44·4....	8	17	55·64	19	25	31·8
	2	5	25	21·7....	8	21	40·86	19	52	45·5
	6	5	16	14·2....	8	36	35·18			
	10	5	25	52·9....	8	51	41·28			

The comet was exceedingly faint and the observations are not considered good. On several days the only possible way of observing it was by using the field of the eyepiece as a ring-micrometer.

'Bulletin Astronomique' for November contains a discussion of the orbit of Finlay's Periodic Comet for the three returns 1886, 1893, 1899, by M. L. Schulhof. The following are the elements for 1899:—

Epoch 1899 Oct. 16·0 Paris M.T.

M	341° 24′ 7″	
ω	315 40 50	1899·0.
☊	52 23 7	
i	3 2 54	
ϕ	46 19 22	
μ	541″·235	

There is, unfortunately, no hope of seeing the comet at this return, as it is very unfavourably placed. But at the next return, in August 1906, it will be only some 18 millions of miles from the Earth, and should be conspicuous. After that it will make a close approach to Jupiter, and the orbit will be considerably modified.

The same publication contains an interesting article by M. L. Picart on a method of deducing a parabolic orbit from three observations of a comet without any previous assumption as to its distance being required.

There is in the same publication a list of all the principal works dealing with the computation of orbits from Newton to the present day. A. C. D. C.

MINOR PLANET NOTES.—The new planet EX, discovered by Mr. Coddington on Oct. 2, appears to belong to that group whose period is approximately one-half of Jupiter's.

Another new planet, EY, of the 10th magnitude, was discovered by M. Charlois at Nice on Dec. 4.

The orbit of the new planet ER shows some resemblance to those of the lost planets (99) Dike and (155) Scylla. Identity with one or other of them is not impossible.

Another planet, EZ, was discovered by Mr. Coddington at the Lick Observatory in October, but this is considered to be almost certainly identical with planet (415), which was recently sought for by Palisa without success. A. C. D. C.

OBITUARY.—NATHANIEL E. GREEN. The subject of this note, whose death was briefly announced in our last number, was born at Bristol on August 21, 1823, the third son of Benjamin and H. Green of that city. At the age of 17 he began to earn his living in a merchant's office in Liverpool, but finding this life not to his taste, and having a love for drawing, he came to London in 1844, and entered as a student at the Royal Academy. He married in 1847, and about a year after marriage settled down in St. John's Wood, where he lived for 49 years. Besides painting in oil and water-colours, he pursued the profession of an art teacher, and had the honour of numbering Her Majesty, the

Princess of Wales, and other members of the Royal Family among his pupils.

His astronomical work, which may be said to have begun in 1859, consisted chiefly in making drawings of celestial objects. His drawings of Mars, made at Madeira during the opposition of 1877, are considered by many the most trustworthy pictorial descriptions of the planet ever made. Twelve of his forty-one sketches were reproduced in the Memoirs R. A. S. vol. xliv. Another important contribution to astronomy by Mr. Green is his memoir on Jupiter (Mem. R. A. S. vol. xlix.), which gives results of his observations of the planet from 1859 to 1887, and deals with the general changes on the planet in these years. The memoir is illustrated by a series of beautiful drawings. His studies of Mars at the opposition of 1886 were not published, but his sketches are preserved in the rooms of the Royal Astronomical Society.

Mr. Green was elected a Fellow of the Royal Astronomical Society in February 1875, and served on the Council for the year 1888–89. He was President of the British Astronomical Association from 1896 October to 1898 October. Of late years he has not been seen much at the meetings of the Society, as, partly for the benefit of the health of Mrs. Green, and also for the purposes of his art, he has spent his winters at Cannes.

Not long before his death Mr. Green left St. John's Wood to live at St. Albans, where he died, after a short illness, on 1899 November 10, at the age of 76, leaving a widow and several children.

We are informed of the death of Mr. Henry James Carpenter*, who was a member of the junior staff at Greenwich about thirty years ago. Mr. Carpenter left Greenwich in 1874 to take up the position of Assistant at the Durham University, which he held until a few years ago. His son now occupies the post at Durham. Mr. Carpenter died on Christmas Eve, 1899, aged 49.

The Zero Meridian for Universal Time.—The claims of Jerusalem to be adopted as the prime meridian of the world have lately been revived by M. Tondini de Quarenghi, an old advocate of this point. His chief objection to Greenwich seems to be that the climate is there so bad that star-observations are impossible there on a large number of nights in the year. We are bound to say that we have never yet felt this to be a sufficient reason for making such a radical change as is suggested.

Spurious Daylight Observations of the Leonids.—Curious instances of the effect of the imagination on the senses have been related in *Nature* (Dec. 7) and in other places. A young lady in Wiltshire, looking out of a window in the afternoon of Nov. 15, thought she saw the air full of small bodies, which she supposed to be the Leonid Meteors, and on mentioning this to three other members of the household, these persons also convinced themselves that they were looking at a meteor-shower. Another correspondent in Essex made a similar statement, as did also a lady of Liverpool, who wrote to a local paper. Mr. Denning, writing in *Nature* of Dec. 14, says he has not the slightest doubt but that these observations were quite imaginary. Such effects can be easily produced by bending the neck and gazing intently at a bright sky. In two of the cases mentioned, the objects, if they were such, could not possibly have been Leonids, as the radiant-point was below the horizon at the time of the supposed observations.

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* Mr. H. J. Carpenter was no relation to Mr. James Carpenter, who died in October last.

THE BEGINNING OF THE CENTURY.—The Berlin correspondent of the *Standard* writes that the Federal Council has decided that the new century is to begin throughout Germany on 1900 January 1, and that the event is to be celebrated by special ceremonies. Since, however, there are in Germany many persons, as in other countries, who hold that the century does not begin till 1901 January 1, the paper war on the question is being vigorously revived. It is announced also that in Sweden the year 1900 is to be considered as belonging to the tweentieth century.

THE LUNAR ECLIPSE OF DECEMBER 16.—The almost total eclipse of the Moon was observed under favourable conditions at Greenwich. As to the colour of the eclipsed Moon, the phenomenon may be considered normal, for at greatest phase the part of the disk in the shadow appeared of a dull copper, darkest and inclining to red in the centre. The unobscured part at this period appeared unusually brilliant, and larger than was to be expected from the computed magnitude, which was given in the 'Nautical Almanac' as 0·005 of the whole diameter. These effects were no doubt due to contrast and irradiation. At earlier and later stages of the phenomenon the part of the disk in the shadow appeared darker than it did at greatest phase; in fact from a black or dark grey just after first contact, it passed through various shades, being at one time almost dark red, until it reached the dark orange or copper of greatest phase, after which the colours recurred in reverse order. At Greenwich four telescopes were employed for observations of occultations of faint stars during the eclipse. Observations of 19 of the phenomena given in the list in the *Companion* and some others were obtained, most of them by two or more observers, making 74 observations in all.

Erratum.—Mr. Lynn asks to call attention to the following in his letter of 1899 Nov. 1, published in our December number:—Page 441, line 14, *for* "much" *read* "not."

Errata in the *Companion*, 1900.—We regret that by inadvertence the epochs of Minima for Variable Stars are given in Greenwich *Civil* Time. To make these continuous with the year 1899, 12^{hrs} must be subtracted from the times given. In the Table of Magnetic Elements on p. 28, last line, *for* 67° 40′ *read* 67° 10′.

Prof. H. H. Turner will give three lectures on "Modern Astronomy" at the Royal Institution before Easter.

The next Meeting of the Royal Astronomical Society will be on Friday, January 12; of the British Astronomical Association on January 31.

From an Oxford Note-Book.

The Royal Society dines on St. Andrew's Day. There is a preliminary meeting to hear the President's Address, and to present medals to those whom the Society delights to honour; but the real event of the day is the dinner in the evening at the Whitehall Rooms of the Hôtel Métropole, when distinguished visitors are present and there are speeches. It has been remarked in previous years that the *Times* report of these speeches seems to be drawn up with a view to the exclusion of anything interesting that may have been said, and the present year was no exception to the rule. There was a veil of gentle dullness over the whole proceedings, possibly due to untoward events in South Africa; but Lord Rayleigh told an excellent story which was apparently considered unfit for publication. He replied to the toast of the Medallists, having himself received the highest honour the Society can bestow, and after most gracefully doing justice to his brother Medallists, modestly remarked that his own interest in science had at any rate begun very early, for at the age of five he had bothered his nurse to tell him why two locomotives were ever put on to the same train. "I argued in this way," he said. "Of the two, one must be faster than the other, and that being the case, what was the good of the slower one?" The Chinese Ambassador also made an excellent speech in English, and this is a specimen of what the *Times* said he said: "No one could predict the new gifts which the Royal Society would confer on the human kind." What he actually said was (and the beautiful quaintness of it is worth keeping intact): "With regard to the new gifts which this great Society is going to confer on human kind at large, nobody could predict."

Concerning the ten-day Ephemerides of stars in various Nautical Almanacs, has it ever occurred to anyone to use second differences in the interpolations? This point came up for examination recently, and I was surprised to find that the use of first differences only, which seems to be assumed as the proper course in the "Explanations" to the Nautical Almanacs, is practically equivalent to a diminution of the Constant of Aberration by 0″·05. This throws a curious side-light on the decisions of the Paris Conference of 1896. After a good deal of discussion the Constant of Aberration was altered from 20″·445 to 20″·470; and yet no one seemed to be aware that the usual practice of computers is equivalent to the use of a constant less than 20″·40. If anyone likes to verify the fact he can do so by various methods, of which the following is one:—Take the R.A.'s of any star as given in the ten-day ephemerides and interpolate in the ordinary way for the second day, getting a new series of ten-day values; repeat this process five times, which should bring back the original numbers if the process is exact; if, on the other hand, the process is faulty, we shall get in the five operations five times the error of one operation. This error will be found to be 0ˢ·02 (equatorial) where aberration has its maximum effect, so that in each operation we make an error of 0ˢ·004 or 0″·06 (approximately).

Of course the error considered in the preceding paragraph is a small one, but it is larger than many quantities which have been very seriously discussed. It can be avoided *either* by the use of second differences in the interpolations *or* by the use of an increased constant of aberration in the ten-day ephemerides. If these are calculated with a constant increased (for this purpose only) by 0″·05, then the use of them with simple interpolation will be equivalent to the use of the non-augmented constant on the average—*i. e.* those who prepare the ephemerides could in this way save those who use them the trouble of using second differences. This would mean that if 20″·47 is adopted as the true constant of aberration, then 20″·52 should be used in forming the ten-day ephemerides. At first sight the device seems arbitrary and misleading, but I suggest it for consideration. I don't think it is a bad plan on examination.

While on the subject of avoiding small errors by conventions, I should like to mention a suggestion made by Mr. W. E. Cooke, Director of the Perth Observatory (W. Australia), with regard to raising figures. The usual practice is to "raise" the figure 5 and all above it; thus ·035 is written 0·4 when two figures are retained, and ·045 is written ·05. This involves, as is well known, a small systematic error. The mean of the twenty quantities ·030, ·031, ·032, &c . . . ·048, ·049, is ·0395; but the mean of their "raised" representatives, viz. five times ·03, ten times ·04, and five times ·05, is ·0400, which exceeds the accurate result by ·0005. Now Mr. Cooke suggests that we should not raise *all* the fives, but only half of them, which would give the correct result, and his rule is "always make the last digit *even.*" Thus,

·035 = ·04, and ·045 = ·04. It would do equally well, of course, to make the last digit always *odd*; but there is no reason to alter his definite proposal, which seems a sound one.

A fine volume recently published by Dr. L. de Ball, Director of the von Kuffner Observatory at Vienna, contains, among other excellent work, two papers by Dr. Schwarzschild on photographic star-magnitudes. He adopts the plan (which has been for some years tried independently at the Harvard Observatory: I am flattered to find that this fact first became known to Dr. Schwarzschild through the medium of these notes in 1896) of taking stars *considerably out of focus*, and estimating the blackness of the discs. His work seems good, but, to tell the honest truth, I have not had time to read his papers completely; may I add that they run to something like 100,000 words—German words—plus 40 huge pages of tabular matter? The letterpress alone is a good deal longer than Prof. Darwin's recent book on Tides, and not much shorter than Miss Clerke's 'History of Astronomy,' and these are in English. I have looked with some anxiety for a summary of the memoirs, but without success. Having made this confession in the first instance, if I say that the author's treatment of simple facts seems rather ponderous—he brings a heavy battery of integration and formulæ to bear on one or two elementary principles—I leave it open for him to reply that this is the shallow criticism of one who has not read his papers.

Although the Leonids were given up as a bad job some weeks ago, there are still sporadic stories concerning them. One was sent me with a definite request for "polishing up," but I find myself unable to add any embellishments and prefer to repeat it *verbatim*:—

> A lady, on November 15 last, went into a second-rate grocery at Rathmines, near Dublin, and while the shopman was tying up a parcel of sugar, remarked that she hoped the night would be fine for the expected meteors. After cogitation he replied, "It is my belief, ma'am, that the public will be very hard to please in the matter of these shooting-stars"; and on being asked the reason for this possible fastidiousness, he explained, "Well! you see, ma'am, the fireworks now are so grand."

Another correspondent, from the north of England as in last month's notes, sends the following advertisement:—

> LOST, STOLEN, OR STRAYED.
>
> A family of Leonids who left home in 1866, and have not since been heard of. They were expected to return to the Earth during the past week, but no trace of them has been found and the worst fears are entertained. Anyone having information as to their whereabouts is requested to communicate at once with the Astronomer Royal, whose anxiety is almost more than he can bear.

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 January 12.

Prof. G. H. Darwin, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. 66 presents have been received since the last Meeting. First, Dr. Ambronn has presented to the Society a copy of his valuable work on astronomical instruments, which consists of two large volumes, and is probably the standard treatise on the subject. Another present received will be of great interest to many Fellows, the second volume of Dr. Roberts's 'Photographs of Stars, Star Clusters, and Nebulæ'; this was only received to-day. The Paris Observatory has also sent 19 charts, each being an engraved enlargement of a chart-plate of the Astrographic Chart; one of these has been put on a board by Mr. Wesley to show Fellows the nature of the reproductions. The original plates are enlarged about twice linear. We have also received from M. Bischoffsheim the first volume of the 'Annals of the Nice Observatory,' and from the American Nautical Almanac Office a copy of Newcomb's 'Tables of Uranus and Neptune.' Mr. W. E. Wilson has sent us a photograph of the nebula in Cygnus H. V. 14.

A vote of thanks was accorded to the Donors.

Mr. S. A. Saunder (reading a paper on the Determination of Selenographic Positions and the Measurement of Lunar Photographs). My object is to call attention to the uncertainty in our knowledge of the positions of lunar formations, and to suggest methods for finding them more accurately. Some 160 points only have been found by methods having any pretensions to accuracy, chiefly by the labours of Lohrmann, Mädler, and Neison, and these have all been measured from the limb. Mädler estimated that a single determination of this kind might be in error by 8″, and less than half the number have been measured as often as ten times. It is well known, too, that measures from the limb are subject to a personal equation not included in Mädler's estimate. The reason why so few have taken up the work is probably the tedious nature of the computations necessitated by the Moon's librations, but Professor Turner has shown that these may be much simplified by the use of rectangular co-ordinates. Greater accuracy may be secured by measuring from some known point instead of from the limb; and Franz's determination of the selenographic position of Mösting A gives a suitable origin. From a short series of such measures of distances ranging from 100″ to 400″, I find that the probable error of a mean position, found from about three-quarters of an hour's work at the telescope, is about one-sixth of a second, this error being determined from the observations themselves. The actual error will often be larger than this, but it indicates a different order of accuracy from that obtained by measures from the limb.

I have also measured some of M. Loewy's exquisite photographs, which, at Professor Turner's request, he was kind enough to send expressly for the purpose. Positive copies on glass, with réseaux, were prepared directly from the original negatives by Mr. Bellamy, and the measures were made with one of the astrographic micrometers belonging to the Oxford University Observatory. A reduction of one of these plates by a method analogous to that devised by Professor Turner for the astrographic-chart plates showed that no set of constants could be found which would connect the measures with the accepted positions of 36 "points of the first order," with any reasonable degree of accuracy, and that the cause of the error was not in the photographic measures. These accepted positions were therefore rejected altogether and the plate-constants found by computation. The chief difficulty was to determine the orientation of the plates: this was finally overcome by a comparison of two, taken under different librations and opposite illuminations, but showing a common zone in the neighbourhood of the first meridian. Expressions were formed giving the selenographic co-ordinates of a point in terms of its réseau co-ordinates and of the unknown orientation. Equations for finding the orientations were then made by equating these expressions as obtained from one plate with those for the same points obtained from the other plate. 84 equations were made connecting the co-ordinates of 42 points, and the solution effected by least squares. It was then found that 67 out of the 84 co-ordinates given by one plate differed by less than half a second from those given by the other. Some of the positions found differed by as much as 15″ from those given by previous observers, but a comparison with the positions of seven points measured at the telescope, as previously described, showed only two divergencies which approached 1″.

The method, therefore, is one of great promise, and with further precautions I hope that still greater accuracy may be obtained.

Amongst the uses to which these measures may be put I would include the preparation of such a map of the Moon as was proposed more than 30 years ago by the Lunar Committee of the British Association, but such as has never yet been made, although it would seem to offer almost the only hope of settling the question of perceptible change on the lunar surface. Dr. Weinek has shown that the height of a lunar mountain can be found with far greater accuracy when the altitude of the Sun can be computed than when it has to be found by measures from the terminator, but this is possible only when the position of the mountain is known with some approach to accuracy. And, lastly, the accuracy with which these photographs can now be measured suggests that they may be available for yet another investigation of the Moon's physical libration, though that is hardly a problem for an amateur.

The President called upon Prof. Turner to say something on this subject.

Prof. Turner. I do not know that I have much to say beyond expressing the great pleasure with which I have heard Mr. Saunder's paper. I should briefly sum up the situation, I think, by saying that he seems to me to have found a regular mine of wealth. I had not the least idea that lunar topography was in such a backward state as it apparently is, and I trust I shall not be too sanguine in saying that it would seem easy to put it right. Mr. Saunder really seems to have got hold of a way to get some accurate places on the Moon, and I must congratulate him upon having been able to do this in the intervals of his hard work as a schoolmaster. I am glad to hear him speak so well of the micrometer. He has found a new virtue in the scale of the astrographic micrometer, and I should like to say that the precise form of scale which he has used was not proposed by me, but by the Astronomer Royal.

The President. Can Mr. Saunder tell us with what degree of accuracy the ephemerides of the libration are now computed? I think there is an ephemeris in the *Nautical Almanac* which gives the libration.

Mr. Saunder. Particulars are given from which the optical librations may be determined, but the only place from which the physical libration can be obtained is the *Berliner Jahrbuch.* The only other ephemeris is that published by Mr. Crommelin in the *Monthly Notices*; in this both the optical and physical librations are included.

The President. You mean the libration due to the unequal motion in the orbit and to the motion in declination, the other part being due to the actual oscillation of the Moon.

Mr. Saunder. The real oscillation of the Moon.

Mr. Crommelin. The accuracy of Mr. Saunder's method suggests that it might be worth while to carry the libration ephemerides to another decimal place. They are given to a selenographic degree. But perhaps it would be as well to go to three decimal figures instead of two. If the figures were given to three places it would certainly be necessary to use second or even third differences. Possibly it will be as well in future to give another figure. I shall be glad to hear what Mr. Saunder thinks.

The President. Does not the estimated position of the centre of a crater vary with difference of illumination? If the crater is illuminated from the right or left it would seem that there is danger of a sort of systematic error in that case.

Mr. Saunder. It is difficult to determine, but that is where the advantage of the Astronomer Royal's scale comes in. With regard to what Mr. Crommelin says, it is the beauty of the method of measuring from a fixed point that the librations need not be known with great accuracy. The value of the libration I use is accurate to 0·1 of a degree, but not to 0°·01 I think.

Mr. Rambaut. If a crater is elliptical, which point of it do you measure?

Mr. Saunder. There is a difficulty in estimating the centre when the crater is not circular or not near the centre, but I hope my measures show that it is a difficulty that can be got over.

The President. Mr. Saunder's exposition has shown us with what consummate ease his formulæ are applied to his measures, but I think none of us were so credulous as to believe he has done all this without very heavy labour. I am very ignorant of the selenographical methods in use hitherto, but I am under the impression that Mr. Saunder has hit upon a method which is an advance upon all previous methods. I am sure I shall express the feeling of the Society when I offer our congratulations to him on his interesting communications.

Mr. Wesley then showed a photograph of the nebula H. V. 14, taken by Mr. W. E. Wilson, of Daramona, West Meath, with the 24-inch reflector, with an exposure of 1½ hours. He believed that Mr. Wilson ordinarily used the Cadett plates. The object had previously been photographed by Dr. Roberts. The nebula was singular because of its streaky character. There were several lines which seemed at first accidental, but it was clear that all those lines were true parts of the nebula. The slide did not show these so well as the positive print. Altogether the nebula seemed to be of a character of its own.

Mr. Dyson (showing a photograph of the Pleiades taken at the Royal Observatory, Greenwich). I am only responsible for the faults of this photograph. You will see the stars have unfortunately trailed a little, which is due to a slight accident I had at the end of the exposure; but the photograph shows the nebulosity of the Pleiades well, and it can be seen better on the negative than on the screen. The exposure was just short of three hours. One interesting point about the photograph is that the guiding is done with the 6-inch refractor mounted on the reflector. I mention this because some people question whether it would be feasible to guide except on the mirror itself, as it was supposed the mirror might shift. The photograph was taken on the last day of 1899.

Prof. Turner. I believe the gentleman who questioned the feasibility of guiding the mirror by means of a separate telescope is a gentleman whom we are glad to see here now, and that is Dr. Common; and I should like to ask him to defend his position.

The Astronomer Royal. Perhaps I may be allowed to say a word before Dr. Common, to show what his real position was. It should be explained, in the first instance, that this photograph was taken with the 30-inch reflector which was made by Dr. Common, and the merit of these photographs is largely due to him. But there is just this point that Prof. Turner has brought out, namely, that the smallness of the star-images is very much due to the stable mounting of the mirror. I think it is due to Dr. Common to say that that is the very point he has always urged; he has said that the reflector, if properly mounted, which has rarely been the case until lately, is as capable of good work as the refractor. The special interest of these photographs we have been taking at Greenwich is that we have been comparing the reflector and the refractor both equally well mounted and with the same clock-work. Dr. Common has for a long time been urging that the reflector has not had justice done to it, because it has been put upon a cheap make-shift mounting instead of on a first-rate mounting as has been the rule with refractors.

Dr. Common. After what the Astronomer Royal has said, I do not think I have any remark to make.

The President. We have present in this room to-night a great authority on stellar photography whom we are all glad to welcome, namely Prof. Barnard. Might I suggest that possibly he would enter into the fray and tell us the rights and wrongs of this disputed question?

Prof. E. E. Barnard. I have not had any experience of a reflecting telescope, but I have generally had the impression that it is not at all comparable with the refractor, especially for visual work; but the things that have been done in the last few years in the way of astronomical photography with the reflector have led me to think that when properly mounted, as Mr. Christie has said, it is the better instrument for taking nebulæ. It is far better than the refractor for that work. I was much interested in this very beautiful picture of the Pleiades, and there is one thing I was rather struck with. I do not know exactly what is the reason, but there seems to be a total absence in this particular picture of the objectionable rays from the brighter stars that are seen in photographs made with the reflector. I do not know how these are got rid of, but I believe it is due to the supports of the small mirror. I should be glad to have a little more information on the subject. I should like to know why the bright stars in the field are not accompanied by nebulosity and how these rather objectionable defects have been got rid of.

Dr. Common. There is only one point I wish to draw attention to after the very delightful manner in which the Astronomer Royal has vindicated my position. There is one thing in that photograph which I should like to speak about, and that is the line that follows the stars, which we have been told was due to an accident in taking the photograph. For some reason or another the clock was stopped a few seconds before the exposure was completed and naturally the brighter stars leave a trail. Now you see how beautifully those little trails decrease and ultimately disappear in the case of two stars that are almost comparable. You can see several stars there that are almost of equal brightness, and yet one has a trail and the other has not. It appears to me, looking at this, that it might be possible to work out a system of measurements of star-magnitudes much more effective than the measuring of the discs. There is one thing I must say. Some 15 or 16 years ago I drew a picture of the nebula in the Pleiades, and I gave a drawing showing the nebula between Maia and Alcyone, and it was exactly as it is represented here. I think it would be a grand thing if we could go somewhat on the lines of the ordnance map and have contour-maps of the heavens—I mean, we ought to have a series of photographs of the same region of different exposures. From a photograph taken with the exposure necessary to just show the brightest stars we should have photographs of gradually lengthened exposure until we came to a picture showing the greatest nebulosity. Looking at the picture now on the screen, the whole of the detail around Alcyone is lost; but if we had a series of photographs taken which could be rapidly displayed on the screen we could see from the beginning how the nebula grew with a longer and a longer exposure.

The Astronomer Royal. I should like to make a few remarks especially upon the point that Dr. Common has raised about contour-lines, because I feel very strongly the value of photography in enabling us to get these contour-lines, by means of photographs of different exposures. A photograph of the Pleiades was taken, by Mr. Davidson, with a shorter exposure, which would have shown Dr. Common exactly what he wished. It shows Alcyone and all the known faint stars about it and one or two stars which he did not record with his 36-inch reflector. The interest of that picture lies in its showing the faint stars around Alcyone, and also the building up of the nebula, as to which photography gives us such great assistance. Prof. Barnard asked a question about diffraction effects. This photograph does not show any great diffraction effects, and we do not happen to have here to-night any lantern-slide that does. We have, however, a photograph of the Orion nebula, though not one that can be put upon the screen, which will show the diffraction effects that are given by the 30-inch reflector. It is to be noted that in taking these photographs we have in the principal focus of the reflector a square plate and plate-holder, and it struck me very probably the square blocking out the centre would produce diffraction effects. There is a hole in the centre of the mirror of the Cassegrain which is used for this work which also produces certain effects of that sort. The point I wish to call attention to in this connection is the importance of considering the shape of the plate-holder at the principal focus. I do not see that any very great harm is done by having these rays coming out from the stars. I think it rather appeals to the public when they see the photographs, and I do not think it does much harm to astronomers when they are measuring the position of the centre. In comparing the results obtained by the reflector and the refractor respectively I do not think that these diffraction effects are an unmixed evil.

Dr. Common. I should like to say that in this particular case the diffraction rays that Mr. Barnard has alluded to are, to a certain extent, masked in the brighter stars by the trail. The effect of the single support is to produce a line across the star. In this case it happens to be that that line across the star would be in the direction of the trail, and we see it shown there, on the side of the star where there is no trail. The trail has nothing to do with producing this. On the other side the trail is slightly intensified no doubt, because if nothing had happened to the clock you would see a line due to diffraction. There is only one support. This does not vitiate what I said just now—that this trailing of stars is very important and might be made great use of. There is a distinct difference between these two [pointing to screen] trails; it would be hardly possible to estimate the difference of brightness with the eye. It is a distinct advantage to have a trail.

Prof. Barnard. Dr. Common has answered the question I asked, and that is how those rays were got rid of. I see there is a faint ray left, but I have been accustomed to see 4 or 6 lines radiating from the images of bright stars.

Dr. Common. It is the fact that two supports produce four rays, three produce six, and four supports produce four. So you can have four supports with less injury to the image than with three.

The President. Is that not because they overlap?

Dr. Common. That is so.

The Astronomer Royal. I think I may state that I do not think this photograph can be taken as representing the diffraction rays that we get on our photographs generally, because on every photograph where we have very bright stars, as in the case near the Orion nebula, we get several rays. That does not really affect the character of the central image. I take it that these other rays are due to the square shape of the plate-carrier combined with the support of the mirror. As I have said, we have a photograph of the Orion nebula which will show it. As there may be a little misconception about what I have said, I may say that the mirror which has been used to take this photograph is mounted as a Cassegrain telescope, but we are using it as an ordinary reflector at the principal focus.

Dr. Rambaut then read a paper on the unpublished observations made with the transit instrument and quadrants at the Radcliffe Observatory, Oxford, between the years 1774 and 1838.

The President. It is interesting to find that such excellent work was done at Oxford so long ago. We are perhaps only too apt to depreciate the work of the great men of science who went before, and whose names, perhaps, are forgotten. I desire to express our thanks to Dr. Rambaut for his interesting paper.

The following papers were announced and partly read:—

Col. E. E. Markwick. "Note on the Variable Star η Argûs."

Royal Alfred Observatory, Mauritius. "Observations of Meteors, 1899 November."

W. S. Aldis. "Tables for Facilitating the Computation of the Radius Vector and True Anomaly for Orbits of any Eccentricities."

George Forbes. "Note on the Physical Constitution of the Lunar Surface."

S. A. Saunder. "The Determination of Selenographic Positions and the Measurement of Lunar Photographs."

A. A. Rambaut. "On the Unpublished Observations made with the Transit Instrument and Quadrants at the Radcliffe Observatory, Oxford, between the years 1774 and 1838."

W. E. Plummer. "Observations of the Lunar Eclipse, 1899 Dec. 16, made at Liverpool Observatory."

H. H. Turner. "Some Suggestions for the explicit use of Direction Cosines or Rectangular Coordinates in Astronomical Computations."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made in the year 1899."

Royal Observatory, Greenwich. "Micrometric Measures of β 883 with the 28-inch Refractor in 1899."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Mars, 1900–1901."

The following gentlemen were elected Fellows of the Society:—

Prof. R. N. Apte, M.A., LL.B., Rajaram College, Kolhapur, India.

Maurice Harvey Clark, Lieut. R.N.R., F.R.G.S., Woodlands, Ashburton Road, Croydon.

Rev. P. W. Fairclough, Kaiapoi, New Zealand.

Umes Chandra Ghosh, M.A., Muir Central College, Allahabad, India.

Richard Kilvington Hattersley, 4 Church Terrace, Blackheath, S.E.

Howard Payn, F.R.G.S., 21 Hyde Park Place, W.

THE BRITISH ASTRONOMICAL ASSOCIATION.

The second ordinary Meeting of the tenth Session was held at Sion College on Wednesday, December the 27th, 1899. *Mr. W. H. Maw*, President, in the Chair.

The President announced that Col. Markwick had kindly consented to undertake the direction of the Variable Star Section, and *Mr. Maunder* read the inaugural address which Col. Markwick had prepared.

The President remarked upon the exceedingly practical nature of the address and hoped it would mark the beginning of great activity in the Variable Star Section.

Mr. Petrie read a paper by *Mr. Stanley Williams* entitled "Considerations on the Double Canals of Mars." The writer said that his recent observations had not confirmed the "optical theory" of the duplicated canals, the theory which ascribed observed duplications to the telescopic image being out of focus, either directly by bad focussing or by what had been termed "diplopia." Notwithstanding the unfavourable conditions for observation this year, owing to the small apparent size of the planet, he had been able to see the canal Cerberus continually double for half an hour at a time, except when a wave of atmospheric disturbance swept over the planet and made everything appear confused.

No change of focussing had any effect beyond making the double canal appear indistinct, and ultimately disappear; also by no change of focussing was it possible to make a single canal appear double. Having referred to his previously expressed opinion that the double canals of Mars were merely two ordinary single canals seen apparently close together, Mr. Williams urged that the fact of particular canals being described as double at one observatory and only single at another had really no bearing upon the observed duplication of canals, especially since the personal element entered so largely into all observations of delicate planetary details, the visibility of which was, moreover, dependent so greatly upon the conditions of seeing, which were seldom the same at two different observatories at the same time.

Even the supporters of the optical theory regarded the duplicity of one canal—the Nilokeras—as real. Yet there was absolutely no difference in appearance between the duplicity of this canal and that of many others. There was a double bay well known as "Dawes' Forked Bay," the reality of which had never been doubted.

There was also a double canal—the Gehon—the two components of which ran from the two inlets of Dawes' Forked Bay. It seemed hardly logical to admit the duplicity of the bay whilst refusing to acknowledge that of the canal. In every theory of the kind now in question, the really crucial test must always be actual experiment upon the object itself, and it was clear from the foregoing that the optical theory of the duplication of the canals of Mars broke down altogether under such crucial test.

Some notes on the foregoing paper by *M. Antoniadi* were read, M. Antoniadi contending that it was not possible to "experiment directly on Mars." He pointed out that in considering the double canals as objective realities, Mr. Williams was unable to account for the fact that they appeared at one and the same time single and double to different observers.

Father Cortie thought that the analogy Mr. Williams drew between observing the canals of Mars and double stars was not sound. In the one case one had two distinct bright points upon a black sky, and in the other the faintest markings on what was itself a very difficult object to see. He had often noticed in observing the spectrum that when his eye became tired the lines began to double and it became necessary to alter the focus, and after a short rest the lines were seen to be single.

Mr. Adams said that the eye could often be focussed so as to see a line double, but one line was always darker than the other. He asked whether this was the case with the Martian doubles.

Mr. Goodacre, referring to a remark of Mr. Williams, said that he had on one occasion seen a lunar rill double, but found that the telescope was slightly out of focus, and after adjusting it the rill became single.

Captain Noble said it was noteworthy that all the most wonderful things seen on Mars had been seen, or imagined, when the planet was a long way from the Earth and unfavourably situated.

During the opposition of 1877, that most favourable opposition, the late lamented President of the Association, Mr. Green, observing at Madeira, saw none of these marvels; but when the planet was a long way off the Sun and Earth, and perhaps showing gibbosity, it was surprising what could be seen upon it.

Mr. Holmes remarked that the dark markings on the Moon were much better seen at other times than full, and it was conceivable that the same might happen with Mars. With reference to Mr. Williams's remark about double stars, no one could have observed them much without knowing that they often did double. He had often been unable to tell whether he saw two stars or four.

Mr. Newbegin confirmed Father Cortie's remarks as to the frequent doubling of the spectrum lines.

Mr. Saunder alluded to some experiments of his on the doubling of the webs of the micrometer.

Mr. Crommelin said he could corroborate Mr. Saunder's remarks as to the doubling of the spider-lines in the telescope. He had noted it long before M. Antoniadi suggested that interpretation of the duplication of the canals. He thought Mr. Williams had made out a good case for the actual duplication of some of the canals.

The President said they were very much indebted to Mr. Williams for his interesting paper and also to M. Antoniadi for his note upon it, which had added to the discussion very much. He could confirm what had been said both as to the duplication of double stars and the micrometer web.

Mr. Maunder gave an account of the arrangements that had been made for chartering a steamer for the proposed Eclipse Expedition. He emphasized that the arrangements would fall through unless a sufficient number of passages had been definitely engaged on or before January 31.

Father Cortie presented the eighth report of the Solar Section, which dealt with the sun-spots observed during the year 1898.

A number of photographs of sun-spots and sun-spot drawings from Stonyhurst and Greenwich, together with some of the great spot of September by Mr. Astbury, were thrown upon the screen.

Father Cortie remarked that we had almost reached the time of minimum of solar spots, and observers of the Sun might profitably turn their attention to observing the solar surface and the wonderful changes that took place upon it, even at times of minimum. He described in some detail the phenomena of "veiled spots," which he believed had only been studied by Trouvelot and at Stonyhurst.

Mr. Maunder drew attention, by means of a diagram, to a remarkable irregularity in the year 1897, both in the area of sun-spots and in the change in latitude of their position.

Mr. Goodacre presented a preliminary report on the recent lunar eclipse, and *Mr. J. M. Offord* exhibited a series of twelve lantern-slides of photographs of the eclipse taken at intervals of a quarter of an hour.

ROYAL METEOROLOGICAL SOCIETY.

The Annual Meeting of this Society was held on Wednesday evening, January 17th, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

The Secretary read the Report of the Council, which showed that the most noteworthy event in connection with the Society had been the removal of the Offices and Library from 22 Great George Street to new rooms at 70 Victoria Street. This step was rendered necessary by the acquisition of the former premises by the Commissioners of Her Majesty's Works and Public Buildings for the erection of new Government Offices.

Mr. F. Campbell Bayard in his Presidential Address discussed the Meteorological Observations made at the Royal Observatory, Greenwich, during the fifty-one years 1848–1898, and brought out in a novel way many interesting features in the variability of the various observations of the barometer, maximum and minimum temperatures, relative humidity, direction of the wind, and rainfall.

These were shown in diagrammatic form on the screen by means of a number of lantern-slides. The Address was also illustrated by various views of the Royal Observatory and of the instruments employed.

Mr. G. J. Symons, F.R.S., was elected President for the ensuing year.

The Twentieth Century [*].

YEARS *ab urbe conditâ* (A.U.C.) are years counted out from the supposed date of the foundation of the City of Rome. These years are given according to Varro, the greatest authority on that chronology; but the old calendar of the Romans fitted the years like a bad clock fits the time. The calendar was sometimes too fast, sometimes too slow. Not only was the calendar a bad fit for the course of the years, but it was made a football of by political factions, who, being trusted with its occasional correction, sometimes put it on and sometimes put it back, in order to facilitate their electioneering tricks, and with only secondary regard for keeping the calendar in beat with the course of the seasons.

Some forty-seven years before Christ, Julius Cæsar—the then Dictator of Rome, and one of the greatest men of antiquity—the man who did Britain the service of conquering it twice, and introducing to it the Roman language, laws, and civilisation—had his attention called to the vast inconvenience of this state of the Roman calendar, which was then some three months too fast, and indicated the time of spring when the season was really midwinter. He added some ninety days to the calendar for the year 46 B.C., and made that year of the calendar to extend over 445 days, which, in effect, put back the calendar some 90 days. Julius Cæsar then made the year 45 B.C. [†] to commence on the first of January, on the day of a new Moon. In order also to make the calendar keep better time, he took up the odd quarter of a day by making every fourth year into what we now call a leap-year. This was one of the great services done by Julius Cæsar to the world, and the calendar thus defined with a year of 365 days, but with every fourth year of 366 days, was called the Julian calendar. The seventh month of the year was named Julius, in honour of Julius Cæsar.

The precise length of the real year was not then known; but subsequently the continual revolution of the seasons showed that the Julian calendar was a little too slow. This was because, as we now know, the length of the year is $365\frac{1}{4}$ days *minus* $11^m\ 12^s{\cdot}43$. Every recurring leap-year, therefore, left the Julian calendar nearly 45^m behind time. But the Julian year was very nearly the real year, and it was so vast an improvement upon the former calendar that the years from the 1st of January of the first Julian year (45 B.C.) were regarded as the Julian era. Now, the diagram shows that decade of Julian years which covers the period from the end of the 40th Julian year to the end of the 50th, each year being indicated by a space which represents the interval *in time* covered by that particular year. According to the chronology of Varro, the 45th and 46th years of the Julian era were more or less contemporaneous with the 753rd and 754th years from the foundation of Rome. These years are indicated in the diagram by the two top numbers.

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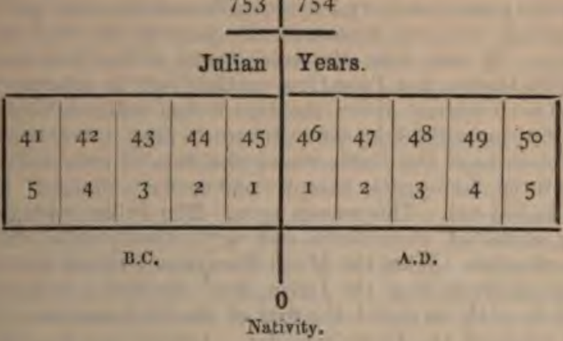
[*] By Dr. James Edmunds in the *English Mechanic*, 1900, Jan. 28.

[†] [709 A.U.C.—EDS.]

Ab Urbe Condita (*Varro*).



The end of the 45th Julian year was the date adopted by the Monk Dionysius Exiguus in the year A.D. 527 as the commencement of the Christian era. It therefore follows that the 45th Julian year was the first year before the commencement of the Christian era, and that the 46th Julian year was the first year after the commencement of the Christian era. The decade of years of the Julian era from 41 to 50 inclusive was thus contemporaneous with the first five years before Christ (B.C.), and the first five years after Christ (A.D.). If, now, the diagram be considered by the light of this explanation, and the spaces be regarded as representing the respective years, it should be easy to see "what it is that we are talking about."

The putting in of the leap-years of the Julian era fell into the hands of the priests. They blundered over it, and for the first 36 years of the Julian calendar they put in a leap-year every third year instead of every fourth year. The priests managed this by something like the method of Lord Dundreary, who discovered that whether he had ten fingers on his two hands—or nine, or eleven—depended solely upon whether he counted his fingers forwards or backwards, and where he left off before he summed them up. Whether the priests began, as some sages of this day do, with a zero year, I do not know. But they counted both extremes, as was the custom in Rome, and so made every three years into four. The result was that in the 37th Julian year, the Emperor Cæsar Augustus discovered that the calendar was too slow by three days, and he then ordered that the next three leap-years should be omitted. Now the next three leap-years would have been 40, 44, and 48 of the Julian era, and these were all reduced to 365 days each. It was not till the fourth leap-year, the fifty-second Julian year, and the seventh of the Christian era, that another year of 366 days was let into the calendar. Thus it came about that leap-years were non-existent in the Julian calendar at the commencement of the Christian era.

Owing to the Julian calendar, with its leap-years, proving too slow for the years by an annual interval of $11^{m}\,12^{s}\cdot43=44^{m}\,49^{s}\cdot72$, or nearly 45 minutes every leap-year, the calendar had got twelve days behind the true season of the year in the time of Pope Gregory. By that time the true length of the year was more accurately known, and Pope Gregory then put the calendar on ten days, so as to correct it for the days it had fallen behind, while, in order to make the calendar in future to keep better time with the revolutions of the Earth round the Sun, he ordained that all the centurial leap-years, except such as were divisible by 400, should be left out. This was in 1582. The Julian calendar, thus further corrected, thenceforth was very unfairly called the Gregorian calendar. What the Monk Dionysius Exiguus had done in 527 was simply to steal the Julian Era. Its first 45 years he cut off, and its 46th he called the first of the Christian era. Otherwise it remained the Julian calendar. And all that Pope Gregory had done was to partly make those further corrections in the number of leap-years which the Julian calendar in its working for centuries had shown to be necessary. The Catholic countries in general at once adopted the New Style (N.S.) of Pope Gregory. This reformed calendar was ten days in advance of other nations, and the unreformed calendar, for the sake of distinction, had to be denoted as Old Style (O.S.). England, being a Protestant country, did not then adopt Pope Gregory's New Style, nor his omitted leap-years. But in 1752 England did this by Act of Parliament (24 Geo. II. c. 23). Russia, being under the sway of the Greek or Eastern church, has even rejected the reformed calendar till this day. Christmas Day, which we held on our Dec. 25, was therefore in Russia the 13th of Dec. Again, owing to the 1900th year with us having its leap-year omitted, while in Russia it is still a leap-year, as ordained by Julius Cæsar, their calendar will be 13 days behind time on their first of March, 1900. Anglo-Russian documents therefore have to carry two dates in order to avoid confusion. The dates will appear thus :—

Thursday, 1900, March $\frac{2\ \text{O.S.}}{15\ \text{N.S.}}$.

Reverting now to our diagram, it will be evident that the years B.C. are merely a retrospective method of labelling time, and that, in counting the years B.C., we count backwards, and in each year we unavoidably begin with the 31st of December, and end with the 1st of January. The years A.D. we count forwards, precisely as we count the years of a man's age, and the early calendar A.D. we regard precisely as we regard the calendar to-day. It is, therefore, evident that the 100th year has to be completed before the 1st century is finished, and that the 1900th year has to be completed before the 19th century is finished. When we come to nineteen hundred *and* one we shall have completed the 19th century and entered upon the first year of the 20th.

As to the sticklers for the precise moment of the real Nativity, we know neither the day nor the month of the Nativity. Historians, moreover, are agreed that Christ must have been born prior to February in the fourth year B.C. But whether Christ were born five years before the date now in use for the commencement of the Christian era, or were born five years after it, or had never been born at all, makes not a particle of difference. Our calendar has been determined by an English Act of Parliament, and it would be alike impracticable and futile for us now to attempt to alter it. Nor does the naming nor dating of our era have anything to do with the number of years which have to be allotted to a century. If a cheque for £100 be misdated, that makes no difference to the number of sovereigns which the payee is entitled to at the bank counter. So, in order to complete a century, we must finish up its 100th year to the last moment.

The Polar Compression of Jupiter.

In A. J. 473 Mr. W. S. Adams deduces the ellipticity of Jupiter from the movement of the apse of the fifth satellite. Though the method is well known, a brief explanation of it (the only one available for Mars and Neptune, as the ellipticity is almost too small to be measured directly) may be of interest.

If the only force acting on a satellite is one varying inversely as the square of the distance and directed to the centre of its primary, the satellite will describe an ellipse with the primary in one focus. The effect of a small additional force, such as that arising from the attraction of the Sun or of other satellites, or from the non-sphericity of the primary, is to change the position of this ellipse; that is to say, the node and apse move. If the inclination of the satellite's orbit to the planet's equator is considerable, or if its eccentricity is considerable, the movements of the node and apse can be observed. Although the eccentricity of the orbit of the fifth satellite is small, Prof. Barnard's observations are of such accuracy that the motion of the apse is clearly shown. As the fifth satellite is very near to Jupiter, the disturbing cause which has most effect in causing this movement is the want of sphericity of the planet.

It might be thought that it would be necessary to know something about the distribution of mass in Jupiter in order to calculate the effect of its attraction on the satellite. This is not the case, however. All that we need to know is the form of the surface and the rate of rotation of Jupiter. For the resultant effect of the gravitation of Jupiter's mass and its rotation is such as to be everywhere normal to the surface of Jupiter; that is, Jupiter's surface is a level surface or surface of constant potential for these forces. This is sufficient to determine the force of Jupiter's gravitation on its satellite, and this determines the motion of the apse. Reversing the problem, the motion of the apse gives the form of Jupiter's surface, its period of rotation being supposed known.

Expressing this in symbols, let ϵ denote the ellipticity of Jupiter, and ϕ the ratio of centrifugal force to gravity at Jupiter's equator; a and a_0 the major axis of the satellite's orbit and the equatorial radius of Jupiter; n the mean motion of the satellite; and $\frac{d\varpi}{dt}$ the mean motion of the apse; then the motion of the apse gives directly the value of $\epsilon - \frac{\phi}{2}$ by the equation

$$\frac{1}{n}\frac{d\varpi}{dt}\left(\frac{a}{a_0}\right)^2 = \epsilon - \frac{\phi}{2}.$$

We are unable to follow Mr. Adams in finding $\frac{\phi}{2}$; it should be derived by the formula

$$\phi = \left(\frac{T}{T_0}\right)^2\left(\frac{a_0}{a}\right)^3,$$

where T and T_0 denote the times of revolution of the satellite and of rotation of the planet respectively, the principle of the formula being that we obtain the force of gravity at Jupiter's equator from its known value at the distance of the satellite.

Taking

$$\frac{d\varpi}{dt} = 2^\circ{\cdot}465 \quad \text{and} \quad n = 722^\circ{\cdot}63$$

(Barnard's values), we find

$$\epsilon = +0{\cdot}7260\left(\frac{a_0}{a}\right)^3 + 0{\cdot}003257\left(\frac{a}{a_0}\right)^2.$$

Taking

$$\frac{a}{a_0} = 2{\cdot}55,$$

we find that

$$\epsilon = 0{\cdot}06496 = \frac{1}{15{\cdot}40}.$$

The uncertainty of this determination of ϵ would appear to arise mainly from the uncertainty in $\frac{a}{a_0}$. The value of a need not be derived from observation, but can be obtained from the mass of Jupiter by the equation

$$M = n^2a^3,$$

and is known with sufficient accuracy. The value of a_0 as shown in the table given below may be anything between 37″·4 and 38″·4. The change in ϵ due to an error in a_0 is given by the equation

$$\frac{\Delta\epsilon}{\epsilon} = 1{\cdot}37 \times \frac{\Delta a_0}{a_0},$$

or, if a_0 is altered by $\frac{1}{50}$th part, ϵ is altered by $\frac{1}{37}$th. The value of a_0 adopted in the *Annuaire* (from which the value 2·55 of $\frac{a}{a_0}$ is derived) is 37″·67. If this value had been increased by $\frac{1}{50}$th and 38″·42 taken, then the resulting value of ϵ would have been approximately $\frac{1}{15{\cdot}00}$.

The following table gives the values of a and $\frac{1}{\epsilon}$ as measured directly in different ways. Both 15·40 and 15·00 are smaller than the values generally obtained. It may be noticed that the more accordant result 15·40 is derived by using a small value for the Equatorial Diameter, such as would arise from heliometer or double-image micrometer measures.

Heliometer Measures.	*a.*	1/ε.
	″	
Bessel……	37·60	15·73
Winnecke …	37·43	16·13
Johnson….	37·38	16·61
Main ……	37·00	18·50

Double-Image Micrometer.	*a.*	1/ε.
	″	
Kaiser……	37·55	11·47
Main ……	37·91	16·92
Lewis ……	37·46	17·83

Filar Micrometer.	*a.*	1/ε.
	″	
Struve ………………	38·33	13·73
Secchi ………………	38·35	15·66
Barnard ……………	38·52	15·98
Dyson and Lewis ……	38·27	16·93

F. W. D.

*An Atlas of Variable Stars**.

A DISTINCTIVE feature of the astronomy of the present day is the systematizing of work in the various branches, by which the effort necessary for attaining results on a wholesale scale is materially reduced. Instances of this are seen in the photographic charting and cataloguing of stars and in the photography of spectra. Lately there has been mentioned in these pages a catalogue of southern double stars which may be hoped to materially help the work of this branch of astronomy, and we have now before us the first part of the Rev. J. G. Hagen's *Atlas of Variable Stars*, which appears to be a unique production, likely to be of the greatest help to the work of those who measure the variation of stellar light.

Review of this work resolves itself merely into description of its details. For this, we have recourse to the author's Latin preface, of which the following may be taken as a free abridged translation:—

The Atlas is divided into five series, the first three of which are intended as an aid to observing variables of the faintest class; the fourth for observing those which are just visible when at minimum by instruments of moderate size; the fifth and last is devoted to those which can be observed by the naked eye. The work consists of a number of charts and pages of catalogue, a chart and a page of catalogue being devoted to each star.

On each chart is found the information that is necessary for the observation of the star to which it refers. First the R. A. and declination for epoch 1900·0 are given, with the annual precession in each coordinate. Then the colour of the star denoted by a number having reference to a scale in which 0 signifies white and 10 means red. To this number showing the colour is added a Roman numeral, giving the spectrum type according to Secchi's classification; next the maximum and minimum magnitude of the star, which are mean results deduced from observations at various times. All these details have been derived from authentic sources, but have in a great measure been verified by Father Hagen. The spectrum types have been taken mainly from the works of Pickering, Espin, and Krueger.

These facts are given in the margin of the chart, which is itself a square, whose sides represent 1°, ruled to form a réseau of squares whose sides are 5′. Thick lines mark an inner square of half a degree side. At the centre of the square the variable star itself is marked, in the interior square are plotted down in their positions all the stars within the region visible in a telescope of 12 inches aperture. In the region exterior to this are similarly plotted all the stars given in the Bonn Durchmusterung, the magnitudes of each being shown by the size of the black circle marking its place. The variable is marked by two open concentric circles, their diameters representing the maximum and minimum magnitude on the same scale as the comparison stars already described, which scale is explicitly given in the lower margin.

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* Atlas Stellarum Variabilium. Series Prima. Complectens stellas variabiles intra limites declinationis −25° et 0° quarum lux minima est infra magnitudinem 10^m. Composita a I. G. Hagen, S.J., Directore Speculæ Collegii Georgiopolitani. Washington. F. L. Dames: Berlin, 1899.

It will be seen that these charts of comparison stars give all data necessary for making observations; at any epoch of its period no doubt a star will be found on the chart of equivalent brightness to that of the variable. For subsequent reduction and discussion, each chart is supplemented by a sheet which gives the B.D. place of the variable and the places of all the stars marked on the chart, indicated by coordinates with reference to the variable as origin, so that each is easily identified. The magnitude of the B.D. is given when the star occurs in that work, and also the difference of magnitude of all the stars from the brightest star on the chart, given in "grades" of an arbitrary scale different from each chart, the differences having been determined by observation. By comparison of the grades with the corresponding B.D. magnitudes of the stars between 7^m and 10^m, a formula is deduced for each plate, and used to extrapolate magnitudes for the fainter stars. The magnitudes of all the stars on the chart thus computed by formula are given. This first series of the Atlas comprises mostly stars in Schönfeld's extension of the B.D., and it is noticed that comparison of the catalogue magnitudes with the magnitudes computed by formula as above described does not show marked systematic difference such as is shown in similar comparisons of Argelander's catalogue in the case of the fainter stars. Added to this are notes stating when a star is suspected to be double, and giving references to other catalogues in which the star occurs.

It is hoped that this detailed description of the work is sufficient to show how admirably adapted it is for the requirements of variable-star observers. This series and a telescope are sufficient, so far as appliances go, for anyone to make a research on the variability of any of the 44 stars here dealt with, which, as has been said, are south stars. The charts are printed in the clearest manner on card of fine texture and thickness, and the pages of catalogue are of equally nice appearance; in fact the Atlas is quite an *édition de luxe.* The expense has been borne (it must be added that the work can be purchased) by that liberal patroness of astronomy, Miss Catherine Bruce; the work has been done by the Rev. J. G. Hagen, S.J., Director of the Georgiopolis Observatory, Washington, who made the observations both of position and of grades, and who gratefully acknowledges help he has received from various persons. All concerned are to be congratulated on the formation of this valuable work, which it is hoped will meet with the reception it deserves.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Variable Velocities in the Line of Sight.

GENTLEMEN,—

Mr. Newall's kind review (*Observatory* for December, 1899, pp. 436–7) of my work on spectroscopic binaries contains many passages of interest to me: especially those recording his possession of a promising new spectrograph, and his independent confirmation of my Capella observations. With large and powerful spectrographs now building, or just completed, for so many observatories, I trust that confirmation of the discoveries made here will soon be undertaken.

In regard to Polaris, Mr. Newall has unintentionally stated the case too strongly in saying that "the recent photographs of Polaris were taken in order to test the current results of the spectrograph," since only the first one of the long series, that of August 8, was taken for this purpose. We are in complete agreement as to the necessity for further tests on "a star whose velocity may be relied on as constant." Such tests were made on two standard stars the very next night, August 9, and on other objects in days following. In addition, all portions of the instrument were carefully examined and all adjustments were tested. In the present advanced state of spectroscopy in general and line of sight work in particular, it did not seem necessary to burden the pages of a technical journal with details of tests so obvious and fundamental. In no. 70, p. 195, *Publ. A. S. P.*, issued a few days before the *Astrophysical Journal* for October, I wrote: "In order to test the matter further, on August 9 spectrograms of Polaris and of β and γ Andromedæ were secured (in direct succession). That of Polaris gave a result of $-10{\cdot}6$ km., whereas those of β and γ Andromedæ reproduced the values obtained for these stars in previous years."

No doubt your readers will be interested in some of the preliminary results for the binary star Capella. The orbits of the two components are nearly circular. Their period is $104{\cdot}1 \pm 0{\cdot}2$ days. The radial velocity of the system is about $+31$ km. per second. The velocity of the solar-type component ranges from about $+4$ to $+57$ km. The velocity of the second component cannot be determined very accurately, owing to the blending of the lines in the two spectra; but a range from 0 to $+61$ km. will not be far from the truth. It will be seen that the two components are not very unequal in mass—a fact of considerable interest if we compare the types of the two spectra.

If a plate secured when the components are moving across the line of sight be superposed on plates obtained when the components have their maximum and minimum radial velocities, the nature of the two spectra appears at once. The principal component's spectrum is essentially of solar type, although the strong background of continuous spectrum from the second component obscures many of the finer details. The second spectrum contains dark lines corresponding to the elements found prominently in the chromosphere: hydrogen, iron, calcium, titanium, &c.

Lick Observatory,
1900, Jan. 2.

Yours truly,
W. W. CAMPBELL.

GENTLEMEN,—

I thank you for your courtesy in sending me the printer's proof of Prof. Campbell's letter, which I received this morning, and can only feel glad that my note has elicited so interesting a reply from him.

With reference to Capella, a preliminary discussion of my own photographs gives the following results:—

Orbit nearly circular.	
Period from my own observations during one revolution, perhaps in error by 0·5 day	104 days.
Period from my observations combined with Campbell's of 1896 (eleven revolutions).	104·0 days.
Radial velocity of system	+27 km./sec.
Range of velocity of solar component......	0 to +54 km./sec.
Components: one of solar type; other of same type as α Persei and Procyon.	
Epochs of corresponding phases: Campbell..	1896 Oct. 18.
Newall ..	1899 Dec. 6.
Interval	1144 days.

It is to be noted that Mr. Campbell's value of the radial velocity of the system is greater than mine by 4 km./sec. This is approximately the amount by which Prof. Campbell's 1899 observations of Polaris differ from his 1896 observations.

Is the discrepancy between our results due to an error in my value? and in that case is Polaris a triple system? Or is the discrepancy due to an error in Mr. Campbell's observations? and in that case is Polaris only a binary system, and are β and γ Andromedæ stars of variable velocity? I am not prepared to say; nor will the following help to a solution of the matter:—

Velocity of α Canis Minoris.

Campbell:	1897 Jan., Feb. ...	−5·0	−4·3	−4·8	−5·0	Mean −4·8
Newall:	1899 Nov., Dec....	−3·3	−3·2	−3·6		Mean −3·4

But are β and γ Andromedæ variable? Consider the following:—

Velocity of β Andromedæ (Campbell, 1896–97).

Date	Dec. 8	Dec. 17	Dec. 24	July 8	July 21
Velocity	+0·8	−0·8	−1·0	+0·6	+2·1

Velocity of γ Andromedæ (Campbell, 1896–98).

Date	Nov. 12	Jan. 5	July 22	July 12	July 13
Velocity	−12·0	−12·8	−10·8	−10·6	−11·2

One wonders whether spectroscopists ought not to cease from congratulating one another on getting over instrumental difficulties and to join in raising a cry against the shiftiness of fixed stars.

Your obedient Servant,

Cambridge, 1900, Jan. 28. H. F. Newall.

The Solar Eclipse of A.D. 878.

Gentlemen,—

In my little work on 'Remarkable Eclipses' I referred to the solar eclipse of A.D. 878, and to the fact that it is in all probability the one mentioned in the Anglo-Saxon Chronicle under the year 879, the year after King Alfred's concealment and subsequent battle with the Danes.

Mr. W. H. Stevenson has recently discussed (in the 'English Historical Review') the dates given about that time in the printed copies of the Chronicle, and found that several of them are one year, and others two years in error. The date of Alfred's death is given as October, A.D. 901, but it is said to be 28½ years after his accession, which is placed in the spring of 871. Mr. Stevenson adduces very strong reasons from other authors for taking A.D. 899 as the date of the king's death, one authority stating positively that his son, commonly called Edward the Elder, was crowned early in A.D. 900. This would make the date in the printed copies of the Chronicle two years too late: but Mr. Stevenson has shown that some of the earlier dates in Alfred's reign are one year too late.

The eclipse is a remarkable confirmation of this, and therefore would seem to prove that the battle of Ethandûn (Edington) and the peace of Chippenham took place in the year 877, not 878 as is commonly given in histories.

Mr. Johnson, in his 'Historical and Future Eclipses,' says that the annular eclipse of March 14, A.D. 880, is mentioned by Asser; but this is evidently a mistake, since Asser's eclipse is placed by him (as by the Chronicle) the year after the famous battle, and evidently is also that of the 29th of October, 878. It is satisfactory then to have this date definitely determined by the eclipse.

Yours faithfully,

Blackheath, 1899, Dec. 2. W. T. Lynn.

Raising Figures.

GENTLEMEN,

In the Oxford Note-Book for January, mention is made of Mr. Cooke's* suggestion on the question of raising figures. He says that the usual practice is to raise the figure 5 and all above it. This may be so, but I have known people somewhat careless even of figures higher than 5. Mr. Cooke very properly suggests that we should not raise all the fives, but only half of them, and he gives as a rule, "always make the last digit even," one that, whilst it has the advantage of being easy to remember, fails on occasions satisfactorily to divide the fives. The problem, as between odd and even, is analogous to that of tossing a coin, in doing which runs of the same face coming up a number of times consecutively will occur. In a set of successive numbers of which the terminal figure may be any digit, those of them having 5 as terminal figure will be found to have a penultimate figure which on occasions is subject to periods in which at one time odd digits, and at another time even digits, will successively preponderate, thus influencing unequally any raising or not raising of the terminal 5. To illustrate this, I have taken at random three tables, each giving values of a different and independent physical element for every day of the year. Each daily value is the mean of a large number of observations, and consists of four figures, all of which are variable, so that in the last two figures of each value there can be no bias towards any particular digit. In the aggregate of the three years we should expect to find a terminal five 109 or 110 times (the tenth part of 1095). They actually number 112. In each of the three tables there should be 36 or 37. In the first they number 33, in the second 36, and in the third 43. But when we come to shorter periods the distribution becomes yet more unequal, as is shown in the following selections made from each of the three tables:—

		Table I.			Table II.			Table III.				
Number of successive terminal fives.	...	6	5	5	9	6	5	4	11	12	5	5
Penultimate figure	even...	1	4	0	8	0	1	0	9	9	0	4
	odd ...	5	1	5	1	6	4	4	2	3	5	1

In Table III. the irregularities are remarkable. As has been mentioned, there are in the whole table 43 terminal fives, and yet they fall into runs of even and odd penultimate in 37 cases, in one case 9 out of 11, and in another 9 out of 12 having the penultimate even.

The matter is really not of great importance, but still it is one that has always interested me, because there are occasions when it is convenient to give attention thereto. Very early in my time it attracted my attention, and my practice has always been to raise the fives *alternately*, as they occur; thus, in a given case, when their number was even, raising as many as not, and when odd, leaving an excess of one only in one or other direction, and I never found any difficulty in acting on this principle.

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* See p. 71.

Yours faithfully,

Blackheath, 1900, Jan. 20. W. Ellis.

Zodiacal Light on Dec. 25.

Gentlemen,—

Did many of your readers notice the singular brilliancy of the Zodiacal Light as early in the season as the evening of Chrismas Day? It was something like this on the same day of December 1869, but not so strong. Width at the base considerable. Further on, the uppermost of the three ψ's in Aquarius was just outside the southern boundary of it, and γ Aquarii on the northern. ω, ι, γ Piscium within it, centrally. About 10° south of the Head of Aries the Zodiacal Light seemed to fade away. It was very diffused; but on looking at the bright patch of the Milky Way below γ Cygni, the contrast was very marked.

Faithfully yours,

Melplash Vicarage, Bridport,
1900, Jan. 4. S. J. Johnson.

A Comet of 1760.

Gentlemen,—

In Mr. Chambers's 'Catalogue of Comets' I find two described as having been discovered in 1760—one "at Lisbon" on Jan. 7th and the other by Messier on Jan. 25th. But if either of these should turn out to be the comet to which I am about to refer, the original discoverer is wrongly given in Chambers's and, so far as I know, in every other catalogue. This comet, according to the *Annual Register* for 1760, was discovered by Mr. Dunn at Chelsea on the 1st of January, 1760. "It appeared to the naked eye like Jupiter or Venus through a thick fog and made a near appulse to the star in Orion's right knee and moved four degrees of the heavens in four hours of time."

The *Annual Register* does not notice the discovery at Lisbon on the 7th, but says that on the 8th "a comet without a tail was seen in Holland in the constellation Orion." The night after this—the 9th—it was also seen in "the 23rd degree of Gemini," when it appeared as a star of the 3rd magnitude. This remark refers to Holland; but Dunn appears to have seen it on the same night and says it passed near μ and ν in Eridanus, going towards the Whale's jaw. It was then cloudy till the 13th, when he could no longer see the comet, and there is no further notice of it in the *Annual Register*. As the editor (I believe Edmund Burke) makes other references to Dunn, the latter was probably a friend of his, and he not only gives Dunn's original discovery at Jan. 1st, 1760, but in speaking of the Dutch observations states that they referred to the comet discovered by Dunn on the 1st of January.

The observer in question was no doubt Samuel Dunn, a native of Crediton, Devonshire, who left a good part of his property to found a mathematical school there. Possibly some manuscript notes of his observations at Chelsea may still be in existence. He lived for 30 years after this date.

It seems strange that a comet which was in Orion on Jan. 1st, and then moving at the rate of 1° per hour, should have been still in the same constellation when observed in Holland on Jan. 8th. Possibly Dunn wrote his 7 like a 1 and thus led to the mistake. But my copy of the *Annual Register* is the seventh edition, published in 1789, and one would have expected that the mistake (if any) would have been corrected in this edition—especially if the editor was a friend of Dunn's. Truly yours,

Dublin, 1900, Jan. 9. W. H. S. MONCK.

The Collimation of a Reversible Transit-Instrument.

GENTLEMEN,—

In determining the longitude between this observatory and St. Louis, in 1888, a difference, amounting to $0^s.07$, was noticed in the collimation as derived from the observations on a distant mark and a solution by least squares of the clock observations.

This was fully confirmed by subsequent investigation, the mean of all the observations giving $0^s.077$; and the cause was attributed to a supposed angular displacement of the object-glass, or tilt out of the perpendicular to the axis of collimation. A difference of 0·0035 of an inch between the east and west sides of the glass seemed to account for the observed results.

After reading the article by Mr. Hollis in the March *Observatory*, the cell of the object-glass was unscrewed, and under one side was inserted a bit of paper of such thickness as to correct about one half of this tilt. A careful determination then of the collimation, both by a distant mark and the solution of the clock observations by the method of least squares, gave a discrepancy of $0^s.03$. Yours very truly,

McKim Observatory, W. V. BROWN.
Indiana, U.S.A., 1899, Dec. 28.

The Comet predicted in 1584.

GENTLEMEN,—

In Mr. Pember's 'Life of Ivan the Terrible' (who died on the 18th of March, 1584) we are told (p. 254) that, "in consequence of the recent appearance of a comet, some astrologers had predicted that his death would take place on that date;" that the Emperor called them impostors, and commanded that they should themselves be put to death, but the order was not carried out because he actually died later in the same day.

With regard to this supposed comet, Pingré remarks, under 1584: "Leovitius avoit prédit pour cette année l'apparition d'une grande Comète; son Astrologie s'est trouvée en défaut."

It may amuse some of your readers to refer to the reasoning which led Leowitz (Leovitius) to predict a great comet in the above year. His tractate, 'De conjunctionibus magnis insignioribus superiorum planetarum, Solis defectionibus, et cometis,' was published at Lauingen in 1564. At the end of it there is a "Prognosticon," on page 18 of which he tells us that in the month of May 1583 there will be a great conjunction of superior planets in the constellation of Pisces, and in the following year (1584) an assemblage of nearly all the planets in Aries—"maxima coadunatio seu coacervatio omnium pene planetarum in Ariete"—towards the end of March and the beginning of April. One of the consequences of this, he predicts, will be the appearance of a great comet—"Ex tali constitutione syderum, cum in signo igneo fiat, Cometæ ingentis apparitionem conjicio, et prodigia varia unde multiplices ac varii effectus à pluribus causis orientur."

We often hear the expression "to take the will for the deed"; but the Russians in the time of Ivan would seem to have taken a prediction for a fact. Any hope, however, of getting rid of that modern Nero must have been acceptable. Yours faithfully,

Blackheath, 1900, Jan. 10. W. T. LYNN.

OBSERVATORIES.

U.S. NAVAL, WASHINGTON.—The Report of this Observatory for the year ending 1899, June 30, is the last which will be issued by Prof. Harkness, as, in accordance with the rule of the service relating to limits of age, he has lately retired. He reports that the 26-inch equatorial has been used, as in former years, for observation of faint and difficult objects and for spectroscopic determination of motions of stars in the line of sight. Especially it has been used in an attempt to determine the effect of irradiation on the measures of diameter of Venus and Mercury, which resulted in the conclusion that the amount of the irradiation depends on the power of the eyepiece employed. For example, in the mean and allowing for variation of the planet's distance, the diameter of Venus with a power of 200 was measured as 17″·99, but with a power of 600 or 785 it was 17″·38. The spectroscope in use with this instrument appears to have given some trouble, and was sent to Mr. Brashear for repair; but it is noted as interesting that several negatives of the spectrum of Capella, taken before the repairs were made, were rejected on account of apparent duplication of the spectrum-lines, which was considered due to defect in the spectroscope. This was before the announcement of the recent researches of Mr. Newall and Prof. Campbell.

Prof. Frisby, who has had charge of the 12-inch equatorial for some years, retired, on account of age, on 1899 May 22. Prof. T. J. J. See is now in charge of this instrument, which is used for observations of asteroids, of occultations, and similar work.

Observations with the transit-instrument have been curtailed, so that only the Sun, Moon, and planets, with the stars necessary for their reduction, have been observed. The new 6-inch steel transit-circle has been brought into use, and also the new 5-inch steel altazimuth, which is to be used (in fact the work is begun) to determine the declinations of ephemeris stars by observations just before and after the meridian in the two positions of the instrument. This altazimuth was also used, in conjunction with the prime vertical transit, in an investigation of the variation of latitude, and a determination of the constant of aberration and nutation.

PUBLICATIONS.

Washburn Transit Observations *.—The meridian-circle (5-inch aperture) of the Washburn Observatory is in use for observations for stellar parallax, but as the method of meridian transits is not refined enough for this purpose, some attempts have been made to increase the accuracy of these observations. The methods have been explained by Mr. A. S. Flint in a note in the *Astronomical Journal*.

(1) *Substitution of moving transit-wire for ordinary fixed reticule.*—The eyepiece is moved by an auxiliary screw geared to the Right-Ascension screw, and turned by either hand, so that the moving wire is always in the centre of the observer's field of view, and a continuous motion is easily obtained. By this means the star under observation is kept midway between two wires, and the screw-head makes automatic contacts at every tenth of a revolution (roughly every two seconds on the equator), with a long contact at the whole revolutions.

It is found difficult at first to use this method of observation, but with considerable practice the results appear to be as good as those obtained in the ordinary manner.

This method is in use at Potsdam and elsewhere, and is said to diminish the difference of personal equation between different observers. On the face of it this is to be expected, as the large differences of personality with fixed wires are due principally to

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* "The Repsold Transit-Micrometer of the Washburn Observatory and Slat-Screen Apparatus." (A. J. 470.)

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the difference between methods (A) and (B) *, and would naturally be much lessened by the substitution of a uniform method for all observers. It is also hoped that the method will practically avoid variations of personality in the same observer; but this is probably too sanguine a view, since they are largely due to causes on which the instrument and method employed have no influence (bodily health, mental or nervous fatigue, and other physiological conditions). There is one case in which the method might modify the result, and that is the variation in the force necessary to make a tap (dependent on the strength and adjustment of the tapper spring), but it is not certain that the automatic contacts will themselves be consistent, judging by the experience of the personal equation machine at Greenwich; it is not surprising to learn that the lever responsible for making the automatic contacts has required very frequent adjustment.

It is doubtful whether the advantages claimed for the method (most of which, as they aim at the diminution of various quantities which disappear in a set of differential observations, seem somewhat unnecessary for the avowed purpose of the observations) can be held to outweigh, or even balance, the great increase of mental strain involved, and the consequent probable diminution of efficiency at far more frequent intervals than is the case with the ordinary method.

The use of two parallel wires instead of one, as is the plan at some places where this method is practised, and the bisection of the interval (10″) by the star instead of the bisection of the star by the wire, is possibly an advantage in the case of faint stars.

It is easy to disconnect the gearing of the eyepiece screw from the R.A. screw if ordinary transits are desired, as would probably be the case for slow-moving stars.

(2) A description is also given of a device for recording bisections in declination, but for want of a diagram it is only partially intelligible. With the exception of an arrangement for recording on the chronograph the time at which a record of bisection is made, which would in some cases be very useful, it does not appear to be very different from methods in use at other places. The range is only 14″, which, as the eyepiece has no motion in declination, is all the particular instrument requires.

(3) Professor Comstock has devised a slat-screen diaphragm to diminish the apparent magnitude of bright stars. An easy adjustment at the eye-end opens or closes more or less a set of five slats somewhat similar to those of a Venetian blind, but made of thin aluminium and not quite touching one another when in the same plane.

These are set perpendicular to the meridian, so that a vertical line of interference images is formed, the central ones remaining

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* These are references to the two well-known methods of observing transits chronographically. See this magazine, vol. xix. p. 212.

as points in all positions, while the external ones quickly elongate into spectra as the slits are narrowed.

It is believed that this will be the most satisfactory of screen devices, and this may very possibly be the case; but all such devices appear to take no account of the fact that the interposition of any diaphragm beyond the object-glass may have an effect upon the line of collimation due to a displacement of the optical centre of the lens, symmetrical portions of which can hardly be expected to be quite concentric for optical purposes.

For evidence of this it should be sufficient to refer to the effect of the pierced cube of the Greenwich transit-circle on the collimator observations. W. W. B.

THE SOLAR PARALLAX FROM TRANSIT OF VENUS OBSERVATIONS.—The transit of Venus of 1882 seems a long time ago, but it is now recalled to memory by the publication, in *Comptes Rendus* of 1899, December 11, of an abstract of a memoir by M. Bouquet de la Grye, in which he gives the definitive results derived from the visual observations of the transit made by the *personnel* of ten expeditions sent out by the French Academy of Sciences.

A provisional result (solar parallax$=8''\cdot78$) was published in 1884, but no details were published. M. Bouquet de la Grye, who had taken part in the observations, and had published two notes on the subject, was charged with the accurate reduction and publication of the observations and was given resources to effect the computation. These are now finished, so far as the visual observations are concerned; the result from measurement of the photographs will be published shortly.

The chief point that had to be studied, and which had led to modification of the provisional result, was the interpretation to be given to the times of contact noted by the observers. It seems that some observers used double-refraction prisms before the eyepiece for the purpose of diminishing the Sun's light; but these at the same time apparently diminish his diameter, and hence retard the time of observed second contact and accelerate that of third contact by as much as 10 seconds it is thought. Observations made with these prisms have not been used. Also the observers were instructed to lightly silver the object-glasses so as to diminish the risk of cracking of the coloured eye-shades by the Sun's heat. This was thought to have affected the first published result, and has now been allowed for. More accurate values of the longitudes of the stations have now been used, and in some other respects the data of computation have been improved.

It is scarcely necessary to say that there are two distinct methods of treating observations of the transit of Venus, known respectively as Halley's and Delisle's. In the first-named the distance between two stations on the Earth which differ in latitude is taken as a base-line; in Delisle's method the base-line is the distance between two stations which differ in longitude. Now the stations to which these expeditions went were all in North and South America, the most northerly and most southerly being separated by 85° of latitude, but no two stations were separated by as much as three degrees of longitude. It is therefore obvious that a better result was to be expected from Halley's method.

This method depends essentially on the determination of the time taken by the planet to traverse a chord of the disk, as found from observations of second and third contacts. Eight observers in the northern hemisphere and nine in the southern observed these. Five observers, two north and three south, had comparatively large telescopes—8 inches aperture—and M. Bouquet de la Grye first takes their observations alone, and combining them into six possible pairs finds for the solar parallax $8''\cdot7996$.

Taking similarly the observations with all the telescopes large and small, and combining each north observer with the mean of observations at south stations, there results $\Pi = 8''\cdot8068$.

Purely as a matter of curiosity, the observations of first and fourth contacts have been treated by Halley's method, and give $\Pi = 8''\cdot783$. The individual results are not very accordant, which is not surprising considering the difficulty of these observations, especially those of first contact.

Coming now to Delisle's method, which necessitates observations of one contact at two stations. Certain observations of third contact not previously used for want of corresponding second contact were available for this. And for similar reasons there are now some additional observations of second contact. The actual number of observations now stand:—Second contact, 10 at stations south of Equator, 12 at north stations. Third contact, 12 at south stations, 8 at north. Combination of the observations of second contact at north stations with those at south gives as result $\Pi = 8''\cdot772$, of third contact $\Pi = 8''\cdot788$.

M. Bouquet de la Grye thinks that the results from second and third contacts, deduced by Halley's method, have a probable error of about $0''\cdot01$, but that the result from Delisle's method is more uncertain. He is therefore led to give as the definitive result from the French visual observations $\Pi = 8''\cdot80$.

NOTES.

Comet Notes.—Ast. Nach. 3614 contains definitive elements of Comet 1897 III. (Perrine) by E. Wessell. The comet was only under observation from 1897 Oct. 16 to Nov. 27. It was very ill-defined, without a nucleus, so that the observations are not in good agreement with one another, and the elements cannot be determined with great precision. The following is the orbit deduced:—

T ...... 1897 Dec. 8·67979 Berlin M.T.

$$\left.\begin{array}{lrrr} \omega \ldots\ldots & 65^\circ & 53' & 58'' \\ \text{☊} \ldots\ldots & 32 & 3 & 9 \\ i \ldots\ldots & 69 & 35 & 58 \end{array}\right\} 1897\cdot0.$$

$\log q$ .... 0·132477

Ast. Nach. 3611 contains a series of observations of Tuttle's Comet (1899 III.) made at the Lick Observatory by Mr. Perrine. On March 9 it was of magnitude 11½, with a nucleus of 16th magnitude. On April 11 it was of the 10th magnitude, 2′ in diameter, with a 12th-magnitude nucleus. The latest observation was on May 1.

In the same publication there is a long series of observations of Wolf's Periodic Comet 1898 IV. made by Prof. Hussey, the dates ranging from 1898 June 16 to 1899 March 10.

Tempel's Comet 1899 IV. was observed on the meridian at the Cape Observatory on 20 nights, ranging from 1899 July 11 to Sept. 9. (Ast. Nach. 3612.) A. C. D. C.

MINOR PLANET NOTES.—Mr. H. N. Russell (Ast. Journ. 473) and Signor Millosevich (Ast. Nach. 3609) have both published revised elements of (433) Eros. The latter are probably the most accurate, as he uses all the observations made in 1898–99, and applies perturbations. We give below (I.) Russell's elements for the apparition of 1898 referred to the equinox of 1898·0; (II.) Millosevich's elements for the apparition of 1898 referred to the equinox of 1900·0; (III.) Millosevich's elements for the next apparition referred to the equinox of 1900·0.

	I.	II.	III.
Epoch	1898 Aug. 31·5 G.M.T.	1898 Aug. 2·5 Berlin M.T.	1900 Oct. 31·5 Berlin M.T.
M	221° 37′ 2″·0	205° 22′ 27″·98	304° 23′ 59″·7
ω	177 39 10 ·6	177 38 39 ·02	177 38 41 ·6
☊	303 29 57 ·3	303 31 46 ·59	303 30 40 ·4
i	10 49 31 ·0	10 49 35 ·30	10 49 38 ·9
φ	12 52 14 ·2	12 52 21 ·99	12 52 48 ·2
μ	2015″·2326	2015″·16324	2015″·12740
log *a*	0·1637876	0·1637975	0·1638027

M. Millosevich gives an extended ephemeris for the next apparition, from which we give the following extract (reduced to Greenwich midnight):—

Date.	R.A.			N. Dec.		Distance from Earth.	Magnitude.
	h	m	s	°	′		
1900 Nov. 10 ……	1	56	17	54	18	0·3743	9·04
18 ……	1	41	4	53	34	0·3534	
26 ……	1	30	17	51	49	0·3379	8·76
Dec. 4 ……	1	26	9	49	18	0·3269	
12 ……	1	29	7	46	17	0·3199	8·55
20 ……	1	38	43	42	59	0·3161	
28 ……	1	54	7	39	35	0·3155	8·45
1901 Jan. 5 ……	2	14	16	36	9	0·3175	
13 ……	2	38	2	32	44	0·3221	8·46
21 ……	3	4	27	29	22	0·3296	
29 ……	3	32	48	26	4	0·3400	8·54

It is interesting to note that Eros made a close approach to Mars on Oct. 26 last, the distance being 0·28, while the least possible distance of the two planets is 0·24. Such a close approach is rare, as their synodic period is no less than 27·59 years, and at most conjunctions their distance apart is considerably greater than the above.

Planet EW proves to be identical with (110) Lydia.

Planet (355) has been named Gabriella, and (387) Aquitania.

A. C. D. C.

The Names of the Satellites of Uranus.—Mr. Forgan, of the British Astronomical Association, is making enquiries into the origin of the names Ariel and Umbriel, the latter being, so far as he knows, a creation of Pope in his 'Rape of the Lock.' How these names came to be applied to the satellites of Uranus is explained by a letter of Miss Lassell to Mr. Forgan, which he publishes in the current number of the B. A. A. Journal:—

"In reply to your query as to the occasion of one of the satellites of Uranus being called 'Umbriel,' I may say that the name was given by Sir John Herschel, to whom my father applied, in order that there might be some distinctiveness to separate these satellites from those that Sir W. Herschel had been supposed to discover, inasmuch as their periods did not agree with any of those four mapped out by Sir William.

"Sir John fixed upon the names Umbriel and Ariel because they were difficult to see, and the one was much darker than the other. I have no doubt the name was taken from Pope's writings, but I do not recollect that this was positively so. Anyhow, the two names quite fit the two satellites, and their names are quite of a similar class.—Jane Lassell."

Observations of the Leonids.—Col. R. Gautier, Director of the Geneva Observatory, has kindly sent us a Report of the results of observations of the Leonid meteors made in Switzerland. There were four stations from which observations were made:— 1. The Geneva Observatory. 2, 3. Two stations in the fortifications of Saint Maurice, where, by permission of Lieut.-Col. Dietler, relays of two soldiers took turns in observing: Savatan (about 700 metres above sea-level) and the fort l'Aiguille (at an altitude of about 1500 metres). At Savatan, one observer looked eastward, the other westward; at l'Aiguille, owing to the configuration of the land, the aspects were north and south. 4. Salvan, on the west side of the valley of the Rhone. Here four observers watched on the night of November 14–15; but Col. Gautier points out that as they did not take turns, but all observed together, their keenness relaxed somewhat towards morning, which accounts for the small numbers at the end of the table for this station.

Date.	Central-European Time.	Geneva.		Savatan.		l'Aiguille.		Salvan.	
	h h	Leonids	Others.	Leonids.	Others.	Leonids.	Others.	Leonids.	Others.
Nov. 14.	11 –12		fog	0	4	0	5	5	1
	12 –13		fog	1	2	9	4	7	1
	13 –14	1	foggy	3	2	9	7	8	3
	14 –15	9	2	7	3	12	2	9	1
	15 –16	8	1	8	1	16	1	14	3
	16 –17	11	foggy	22	0	32	3	36	7
	17 –17½	18	5	20	5	54	7	14	6
	17½–18	2	foggy	31	3	58	5	1	3
	18 –18½		fog	42	2	21	1		
	18½–19		fog	9	1				
	Totals....	49	8	143	23	211	35	94	25

On November 15 between 11^h and 13^h 30^m thirteen Leonids were observed at Savatan, but it was foggy during the rest of the watch. At l'Aiguille on the same night it appears to have been clear throughout, but only 17 Leonids were seen and 8 other meteors. On the next night, Savatan had the clear weather, and 20 Leonids were seen with 3 other meteors. At l'Aiguille it was partially foggy and only 9 Leonids were seen. At Geneva thick fog prevailed on both Nov. 15 and 16, and at Salvan no watch was kept.

M. Beaurepaire, Director of the Zo-Sai Observatory, Shanghai, has also favoured us with a Report of Leonid observations:—

Watch was kept at two stations eighteen miles apart in east and west direction, Zi-ka-wei and Zo-Sai, on the four nights of Nov. 12, 13, 14, 15. On three of the nights there was nothing worthy of notice, but on the night of Nov. 14, or rather in the early morning of November 15 (civil), a few Leonids were seen. The numbers seen during the consecutive half-hours are as follows, the beginning of the watch corresponding to 14^{d} 19^{h} G.M.T.:—

	h m		h m	Leonids.	Non-Leonids.
From	3 0	to	3 30	2	1
	3 30	to	4 0	5	2
	4 0	to	4 30	7	0
	4 30	to	5 0	7	0
	5 0	to	5 30	5	0

M. Beaurepaire adds that all these meteors were remarkable for their brilliancy. Those given as Leonids undoubtedly radiated from the loop of the Lion's head between η and ϵ: some of these were especially dazzling, generally in the case of those shooting towards the horizon; one of such is reported to have burst at the end of its path, which was in a south direction. Equatorially mounted cameras were ready for use at both stations, but it was not considered worth while to use these, as the meteors were so few.

Reports from several American stations are given in the January number of *Popular Astronomy*, accompanied by charts. At no place were an excessive number of Leonids observed; but the largest displays were seen from Doane College, Crete, Nebraska, where 198 meteors, not all of them Leonids, however, appeared on November 14 between 18^{h} and $23\frac{1}{2}^{h}$ G.M.T., and from University Park, Colorado, where 150 Leonids were counted on November 15 between 21^{h} and 24^{h} G.M.T. On the night of Nov. 16 it was cloudy at Colorado; but 28 Leonids were counted at Doane Park.

To these may be added reports from very different longitudes. An officer of a ship in long. 168½° E., lat. 11° S., on the morning of Nov. 15, and in long. 172° E., lat. 7½° S., on the morning of Nov. 16, writes that not more than a score were seen on either morning. In a letter from Nelson, New Zealand, where no Leonids were seen, it is stated that about 3 o'clock in the afternoon of Nov. 15 the roof of a school-house was bombarded with pebbles, which the writer thinks might have been a daylight appearance of the Leonids. He sends some specimens of these pebbles as verification of his statement. It need scarcely be added that the radiant-point is not above the horizon at three in the afternoon.

A BRILLIANT DAYLIGHT METEOR.—On the afternoon of Jan. 9, at 2^{h} 55^{m}, when the Sun was shining brightly in a cloudless sky, there was seen by several persons at widely different places—so many as to leave no doubt as to the actuality of the phenomenon—a brilliant fireball. Most of the reports come from Surrey, Sussex, and Kent, but it was seen also at Maida Vale, London, and from

Dunstable. The general description is that it looked like a ball of luminous metal, and threw off flakes of flame as it went; the duration was estimated at three or four seconds by one observer, but the time is variously given. The observations are summarized (probably by Mr. Denning) in *Nature* of January 25, and from them a real path is deduced, which is given, however, as liable to revision if further descriptions come to hand. "The meteor began, 59 miles in height, over a point 10 miles east of Valognes, near Cherbourg, France. Meteor ended, 23 miles in height, over Calais, France. Length of path 175 miles. Radiant-point 280°, 12°."

Such a phenomenon is rare, but not unprecedented. A fireball of somewhat similar character, but leaving no trail, was seen from Dublin and from observers in England soon after noon on 1894, February 8 *.

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA.—We have been favoured with a copy of the first publication of this Society, which was organized at the conference of astronomers and astrophysicists held at the Yerkes Observatory in September last. The publication is, in some way, a report of this first meeting, giving the Articles of Constitution, the Bye-Laws of the Society, and abstracts of the papers presented at the meeting. These abstracts are excellent reading; they are not too long, but indicate clearly the scope of all the best things done in the way of astronomy in America in the last year or two. Some of these we have already noticed in this magazine. Among those that are new to us is the description of an instrument called a Phonochronograph, which appears to be the barrel of a phonograph used as a chronograph; a clock completes an electric circuit every second or every two seconds in the ordinary way, which causes a hammer to strike a sounding-box, and so marks the phonograph barrel; the observer similarly marks the barrel by his voice. The records on the barrel are afterwards read by eye with the aid of a microscope, or it is possible to read them by ear. The idea is ingenious, but does not appear to have much advantage over other forms of chronograph. These abstracts of papers are printed on fine paper with wide margins, and altogether the Report is an artistic production. We shall look forward to receiving the second volume next year.

SOLAR HALOS AND PARHELIA.—On the morning of Thursday, January 11, between 9^h 30^m and 11^h 30^m, a well-defined exhibition of these solar phenomena occurred. The following description, taken from *Nature* of January 18, appears to be compiled from the statements of several correspondents in Sussex and Surrey:—

"The halos consisted of (1) a first bow concentric with the

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* See this magazine, vol. xvii. p. 112.

Sun, red within, blue or green without, and having an angular radius of 21° or 22°; (2) a second circle or halo, similar to the first, but at twice the angular distance from the Sun; (3) two inverted arches touching the two concentric halos, blue or green on the concave side, and red on the convex; (4) bright patches or mock suns on the same level as the Sun, and apparently lying on the halos. The sky near the horizon was cloudless but misty, the Sun being faintly visible.

A MAGNETIC DISTURBANCE.—A disturbance of the magnets of unusual character occurred on the night of January 19 between 6^h and 9^h P.M. The main feature of the disturbance, as recorded at the Royal Observatory, Greenwich, consisted of a bold decrease of declination to the extent of 28′ between 6^h P.M. and 7^h 30^m P.M., with return of the magnet to its normal position by 8^h 30^m P.M., followed by a smaller wave in the same direction to the extent of 12′ between 8^h 30^m P.M. and 9^h P.M. In the horizontal force trace a number of rapid fluctuations were shown. On the succeeding night (night of January 20–21) other minor movements were shown. Owing to a period of cloud about this date, it is not known whether these movements were accompanied by manifestation of sun-spots.

A COMPARISON OF SOME NATIONAL OBSERVATORIES.—The future of the U.S. Naval Observatory is evidently a matter of some interest just now. In the *Pub. Ast. Soc. Pacific*, no. 71, there is a long article on the subject, containing among other things a comparison of the resources and of other facts relating to certain well-known observatories, which may be interesting enough to reproduce.

The annual income of the U.S. Naval Observatory is $85,000, of the Paris National $53,000, Greenwich $49,000, Harvard $46,000, Poulkova $46,000, Cape of Good Hope $33,000.

The Naval Observatory grounds cover 70 acres, valued at $66,000. The Harvard Observatory covers seven acres near a populous part of the city, valued at $50,000. We can add to this that Greenwich Observatory covers about three acres, but it is difficult to estimate its value, owing to the circumstances of the case.

The buildings of the Naval Observatory are valued at $457,513, of Harvard at $52,000, and those at Arequipa at $12,000. The Greenwich buildings are certainly of more value than the Harvard estimate, and probably less than that of the Naval, but the antiquity of parts of Greenwich might enhance its value if it were *ever* put on the market.

The Naval Observatory has had ten Superintendents in thirty-three years. Greenwich has had eight Directors in two hundred and twenty-four years. Harvard Observatory has had four Directors in fifty-nine years.

THE Ephemerides in the 'Annuaire du Bureau des Longitudes' of former years have in most cases been made to conform to the time in general use—that is, the day was divided into two periods of 12 hours each. The ephemerides of variable stars have, however, been given in accordance with astronomical time reckoning. In the 'Annuaire' for this year a change has been made, and all the ephemerides are given in Civil Time, the hours being numbered from 0 to 23, midnight being 0 hours. It is due to this change that the minima of variable stars in our *Companion* are this year given in Civil Time, as announced in the January number *. This change in the Paris publication has given rise to various newspaper paragraphs of doubtful accuracy. It has been stated that the Paris Observatory is to adopt the same system, and that the 24-hour clock-dial is to be adopted generally in France; but we believe there is no foundation for these statements.

M. CRULS, the Director of the Rio Observatory, published, about a year ago, some very convenient little tables for approximately predicting the circumstances of an occultation at any station. He has now published a more elaborate work, which enables the predictions to be rendered accurate to within a few seconds of time; the computation being assisted by some carefully-drawn curves on millimetre paper, which are contained in the book.

IT is announced that Prof. William Harkness, Astronomical Director of the U.S. Naval Observatory, retired as Rear-Admiral on December 17 on reaching the age of sixty years. Prof. S. J. Brown has been appointed to succeed him at the Observatory.

THE Gold Medal of the Royal Astronomical Society has this year been awarded to M. Poincaré, for his researches in celestial mechanics.

MAY we offer our best congratulations to Capt. W. de W. Abney, who has been promoted to a Knight Commandership of the Order of the Bath.

THE Annual General Meeting of the Royal Astronomical Society will be on Friday, February 9, at 3 o'clock in the afternoon. The next meeting of the British Astronomical Association will be on Wednesday, February 28.

FROM AN OXFORD NOTE-BOOK.

IT will be news to many, perhaps even to the Editors of this magazine, that a contemporary has appeared under the title *The Astronomer*. We may cordially wish it a long life, although its life-history is an unusual one; for as yet it has not published a single number, only a titlepage—nay, not even a whole titlepage, as I shall explain presently. I am referring to the magazine which appears on the stage in the play of 'A Message from Mars' at the Avenue Theatre, which I advise all who like a thoroughly amusing play to go and see. Mr. Charles Hawtrey is admirable as a second Scrooge, who is converted from his selfish ways by a "Messenger from Mars." He falls asleep in his armchair reading a copy of our new contemporary, whose title I could plainly see from the stalls. Curiosity being thus excited I wrote to Mr. Hawtrey for information, and he most kindly sent me the magazine used on the "first night," which proves to be a copy of the *Review of Reviews*, with *The Astronomer* pasted over the title, so that even the titlepage of our contemporary is not its own. As for contents (other than the spurious ones belonging to the effaced title) there is but a scrap of paper, with the words typewritten, which the actor says before he falls asleep. They commence as follows:—

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* We issue a slip with this number to be inserted in the *Companion*.

> Well, now for Mars. That's better. I'm comfortable at last. "Latest observations have revealed strange lights, which some astronomers believe to be signals put out in the hope of attracting an answer from our planet." I don't believe a word of it. The thing's absurd. And yet, &c., &c.

In last month's notes I fear I may have put Mr. W. E. Cooke into rather a false position, by mentioning a proposal for raising the figure 5 as an original suggestion of his own. He did not, in any way, claim it as original; but it was new to me, and I mentioned the source from which it came. It has since been pointed out that the principle is adopted in the *Harvard Photometry*: it also appears in Hayford's 'Geodetic Astronomy' (New York: 1898), p. 271; and perhaps elsewhere. There has, of course, not been time to hear from Mr. Cooke on the subject; but I hope he will not have been unduly perturbed by my mistake. The suggestion is also referred to by Mr. Ellis in another part of this number (p. 95). Since writing the note last month I have used the principle in practice, and find it simple and convenient.

The reference to Hayford's 'Astronomy' in the preceding paragraph was given me by the Director of the Lisbon Observatory, Capt. C. A. de Campos Rodrigues. He very kindly writes to say, in answer to the enquiry whether anyone uses second differences in interpolating from 10-day ephemerides, that it has been the practice to use second differences at the Lisbon Observatory ever since 1878—*i. e.*, since regular observations of Almanac stars have been made. The Lisbon Observatory has published so little recently that this news is very welcome as evidence that good and accurate work is being done there; and we may trust soon to have it in published form. Some of us may be in the neighbourhood of the Observatory next May, "for cause of Skiences," as they said in Japan.

The following postcard was received the other day from an unknown correspondent. To reproduce it *verbatim* seems the best way of helping his project of improving Ambronn's great work; and I do so the more cordially because he makes several polite and even flattering remarks which it is a pleasure to print. Such opportunities are too rare to be neglected :—

> Mr. "Oxford Note-Book,"—
>
> I venture to put before you as a supreme court now of astronomical current appreciation, the enumeration of some slight wants I find in the general execution of Ambronn's 'Handbuch der Instrumentenkunde,' to become much more useful and handy as a reference book : 1. Each cut should bear a *descriptive title* ; 2. If possible also a *legend* of its letters ; 3. The reference letters in the text should be all *in italics* ; 4. There should be some sort of *bibliography*, at least of the works mentioned, such as Loewenherz, Bericht (über was? . . . .), Konkoly, Anleitung. &c. ; 5. All dimensions should be reduced to the *metric system*—some are in French inches, some in English, some in centimetres. Perhaps if you patronize those improvements may they be adopted in some new edition, or in new like work. Wishing a Happy New Year to the *Junior member* and his parent, I really apologize for addressing you anonymously ; but as my name would tell you nothing indeed, I prefer to remain nameless, only as (like everybody, I think)
>
> 1899, Dec. 28. An eager Reader of the Note-Book.

Mr. W. Ellis has kindly sent me the following note :—

> Attention has sometimes been drawn in the Oxford Note-Book to the excessive number of figures employed in giving values of physical contents and in astronomical calculations generally. But astronomers are not the only people who run into extremes in this way. Those who take interest in matters musical will know how imperfect was the violin-bow long after the violin itself had reached a point of comparative perfection—until, indeed, by the genius of the younger Tourte in the last century (by which, perhaps, at this moment I ought to say that I mean the eighteenth) the previous unsatisfactory pattern of bow became superseded by Tourte's elegant construction, that has made of the violin almost another instrument. The Rev. H. R. Haweis, in his lately published very interesting work on 'Old Violins,' gives the length of the bow as fixed by Tourte as between 29·134 and 29·528 inches. What can 0·001 here signify? one would rather think that the length might be a function of that of a man's arm. Two bows that I possess (neither of them a Tourte, unfortunately) have lengths of 29·2 and 29·4 inches respectively.

Three important documents have come to hand this month from the United States—the first report of the Board of Visitors of the U.S. Naval Observatory, the account of the first meeting of the Astronomical and Astrophysical Society of America, and the first Annual Report of the Director of the Yerkes Observatory. In brief notes like these, only the most casual reference can be made to any of them, though voluminous commentaries could be written on each. It is news to me that the resources of the U.S. Naval Observatory are so large—half as much again as those of Paris, Greenwich, and Harvard (which are nearly the same). The new Director, Prof. S. J. Brown, has spent some time in studying the Observatories of Europe, and seems well aware that there is a discrepancy between the resources of Washington and its output [illegible] all good wishes in his endeavours to [illegible] establishment of a Board of Visitors [illegible] Board be not very active (and some- [illegible] of Board), its Annual Meeting acts as [illegible] of the Observatory up to date, take [illegible] and have a thorough clean down; all [illegible]

[illegible] and Astrophysical Society, though in [illegible] counterpart of our Royal Astro- [illegible] to differ from it in several essentials. [illegible] the Society shall maintain a library, and [illegible] with other societies is not expected." [illegible] difficult to fix on any local habitation for the [illegible] general satisfaction. Further, the present [illegible] papers submitted is to print *abstracts* in [illegible] these in pamphlet form for distribu- [illegible] of the complete papers is apparently [illegible] the Society. Now the printing of papers [illegible] the usefulness of the Royal Astronomical [illegible] cause us a little surprise that the new [illegible] anticipate that it will have to under- [illegible] near future, especially as the two dozen [illegible] are given reach a very high standard; [illegible] that the very qualification for member- [illegible] words, "any person deemed capable of [illegible] paper upon some subject of Astronomy, [illegible] branch of physics, may be elected by the [illegible] &c., &c." The contrast between our [illegible] and ourselves is thus complete. When we [illegible] nothing about their ability to write good [illegible] them we print them. "Over there" [illegible] candidate "Can you write a paper?" and if [illegible] and do not undertake to print the result [illegible] no doubt there are excellent reasons for this [illegible] The "Constitution and By Laws" seem admirable [illegible] point. Some phrases sound oddly in English [illegible] appointed by the Chair shall canvass the ballots [illegible]

[illegible] of the Yerkes Observatory is very welcome. [illegible] Prof. Hale has at last got well under way, [illegible] a thoroughly business-like report. The only [illegible] is a paragraph labelled "personal establishment" [illegible] what persons constitute the staff of the observa- [illegible] Prof. Barnard, we are glad to see in England [illegible] for the best portrait-lens in existence, and [illegible] luck in an arduous search.

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of work. We may wish him all good wishes in his endeavours to remove this discrepancy. The establishment of a Board of Visitors is a good move. Even if a Board be not very active (and sometimes this is the best kind of Board), its Annual Meeting acts as an incentive to bring the work of the Observatory up to date, take stock of matters generally, and have a thorough clean down; all of which are most beneficial.

THE new Astronomical and Astrophysical Society, though in some respects the American counterpart of our Royal Astronomical Society, is apparently to differ from it in several essentials. "It is not intended that the Society shall maintain a library, and an exchange of publications with other societies is not expected." I suppose it would be difficult to fix on any local habitation for the library that would give general satisfaction. Further, the present practice with regard to papers submitted is to print *abstracts* in the journal *Science*, reprinting these in pamphlet form for distribution; but the regular printing of the complete papers is apparently not to be a function of the Society. Now the printing of papers is a very serious part of the usefulness of the Royal Astronomical Society; and it may well cause us a little surprise that the new Society does not apparently anticipate that it will have to undertake similar work in the near future, especially as the two dozen papers from which extracts are given reach a very high standard; and we may even remark that the very qualification for membership is couched in the words, "any person deemed capable of preparing an acceptable paper upon some subject of Astronomy, Astrophysics, or related branch of physics, may be elected by the Council to membership, &c., &c." The contrast between our cousins over the water and ourselves is thus complete. When we elect Fellows we ask nothing about their ability to write good papers; but if they write them we print them. "Over there" they carefully ask a candidate "Can you write a paper?" and if he can they elect him, and do not undertake to print the result of his labours. But no doubt there are excellent reasons for this determination. The "Constitution and By Laws" seem admirable—brief and to the point. Some phrases sound oddly in English ears: *e. g.*, "tellers appointed by the Chair shall canvass the ballots thus cast."

THE First Report of the Yerkes Observatory is very welcome. Amid many difficulties Prof. Hale has at last got well under way, and has produced a thoroughly business-like report. The only omission I notice is a paragraph labelled "personal establishment" stating clearly what persons constitute the staff of the observatory. One of them, Prof. Barnard, we are glad to see in England just now; he is looking for the best portrait-lens in existence, and we may wish him luck in an arduous search.

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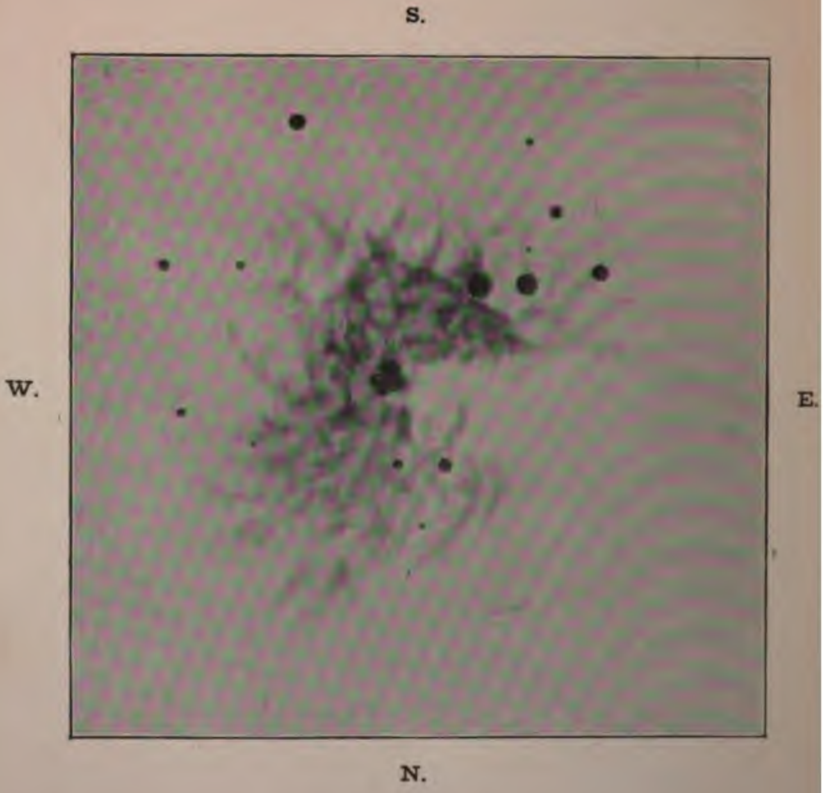


THE ORION NEBULA.

A Drawing made from micrometer measurements of a photograph taken with 1 minute exposure, 1896 January 8.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

[illegible] MARCH, 1900. No. 290.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 February 9.

[illegible], LL.D., F.R.S., *President*, in the Chair.

[illegible]: H. F. Newall, M.A., and F. W. Dyson, M.A.

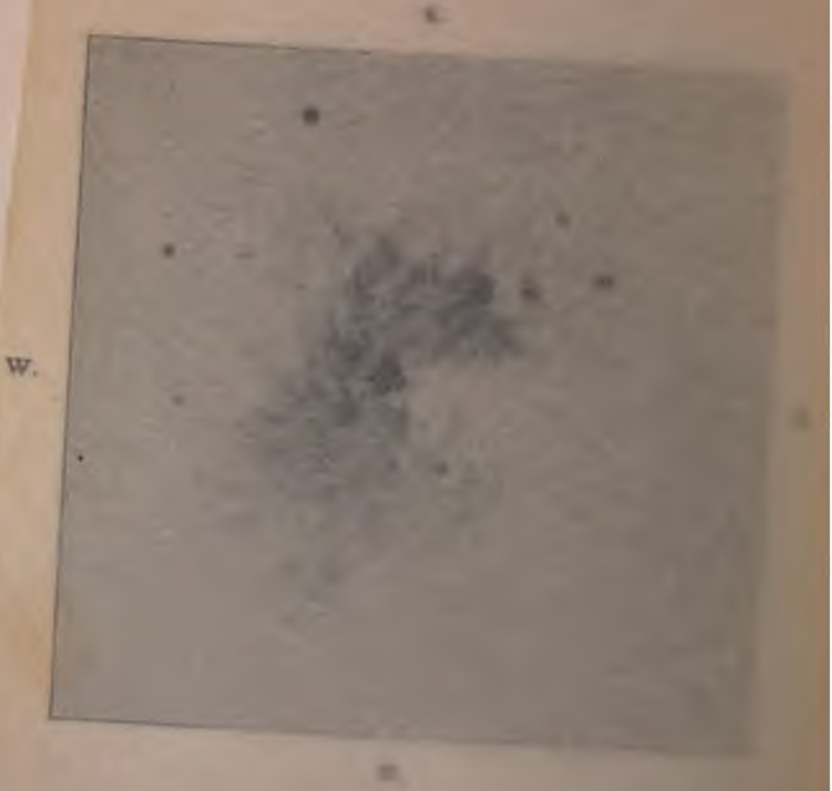
[illegible] read the Minutes of the last Annual Meeting, which were [illegible].

[illegible] read the Report of the Auditors, which stated that [illegible] Treasurer's accounts for the year 1899, [illegible] the same to be correct. The cash [illegible] December 31, including balance at bank, was [illegible] compared with £565 6*s*. 8*d*. on 1899 January 1. [illegible] changes had been made in the [illegible], the particulars of which were set out [illegible] the Report. They had inspected [illegible] and other effects in the possession of the [illegible] to them to be in a very satisfactory [illegible].

[illegible] accounts and the Auditors' report thereon were [illegible] Meeting.

[illegible] from the Report of the Council for the [illegible] had lost by death since the last General [illegible] Fellows:—Lieut. Baillie, Rev. E. L. Ber[illegible] Cooke, Thomas Finch, Nathaniel [illegible] H. T. Moore, John Newton, C. L. [illegible] J. C. Roger, Rev. John Slatter, Hale [illegible].

[illegible] from the Reports of Observatories for [illegible] in illustration of these were thrown on [illegible].



THE [illegible] NEBULA.

A Drawing made from [illegible]

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXIII. MARCH, 1900. No. 290.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 February 9.

Prof. G. H. DARWIN, LL.D., F.R.S., *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and F. W. DYSON, M.A.

Mr. Dyson read the Minutes of the last Annual Meeting, which were confirmed.

Mr. Inwards read the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1899, and had found and certified the same to be correct. The cash in hand on 1899 December 31, including balance at bank, was £434 6*s*. 2*d*., as compared with £565 6*s*. 8*d*. on 1899 January 1. During the year considerable changes had been made in the investments of the Society, the particulars of which were set out in the account accompanying the Report. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in a very satisfactory condition.

The Treasurer's accounts and the Auditors' report thereon were adopted by the Meeting.

Mr. Newall read extracts from the Report of the Council for the past year. The Society had lost by death since the last General Meeting the following Fellows:—Lieut. Baillie, Rev. E. L. Berthon, James Carpenter, Samuel Cooke, Thomas Finch, Nathaniel E. Green, John Marshall, B. T. Moore, John Newton, C. L. Prince, G. C. Pulsford, J. C. Roger, Rev. John Slatter, Hale Wortham.

Mr. Dyson read extracts from the Reports of Observatories for the year 1899; diagrams in illustration of these were thrown on the screen.

Mr. Newall read extracts from the notes on the progress of Astronomy during the past year, which were also illustrated by lantern-slides. The following are the headings of the notes:—Discovery of Minor Planets, Double Stars, Variation of Latitude, Variation of Gravity at the Earth's Surface, the Measurement of Base Lines and Standards of Length, Longitude Determination, Star Catalogues, the Comets of 1899, the Astrographic Chart, Meteoric Observations.

During the reading of these extracts, M. Poincaré, accompanied by the French Ambassador, entered the room and was received by the President.

The President. The Gold Medal of the Royal Astronomical Society has this year been awarded to M. Henri Poincaré. (Loud applause.) We have the honour also to welcome His Excellency the French Ambassador. (Renewed applause.) As your President, the agreeable duty of presenting the Medal to M. Poincaré falls upon me, but before doing so I will endeavour to lay before you the grounds upon which the Council have made this award.

Prof. Darwin gave an instructive address, summarizing M. Poincaré's work relating to rotating fluid bodies, the tides, the figure of the Earth, the problem of three bodies, and periodic orbits. At its conclusion the Medal was presented to M. Poincaré amid the loud applause of the assembly, the President adding that in handing the Medal to M. Poincaré, and in seeking to do him honour, the Society felt itself honoured.

Col. Waterhouse. I think, gentlemen, we have all listened with very great interest and pleasure to the Report of the Council and to our President's Address. I beg to propose that the Report now read be received, adopted, printed, and circulated in the usual manner, together with the President's Address and the Report of the Auditors.

Admiral Sir Erasmus Ommanney. I beg to second that.

M. Poincaré and the French Ambassador now left the Meeting.

The President. We now come to special business. We will first consider the proposal to make an addition to Bye-Law 24. The Secretary will read the proposed addition.

Mr. Newall. It is proposed that the existing Bye-Law 24 be amended by the addition of a Clause. This Bye-Law relates to the resignation of Fellows. At present it reads that a Fellow who wishes to resign shall continue liable to the annual contributions until he shall have discharged all sums (if any) due by him to the Society. The addition which is proposed is this:—"Provided that in the case of a Fellow who, being in arrear of the payment of his annual contribution, should signify his desire to resign his Fellowship, it shall be within the power of the Council, in case of his inability to pay or for any other adequate reason, to remit a portion or the whole of any such liability by a special vote, in which at least two-thirds of those present and voting shall concur."

The President. Will you explain the reason of the proposed addition, Mr. Knobel?

Mr. Knobel. Mr. President, I would like to say a few words to explain the action of the suggested alteration of the Bye-Law. For years past our Bye-Laws have been deficient in one way, that they have not allowed the Council any discretion to deal with certain cases of Fellows who are in arrear with their subscriptions, and whose cases, in many instances, have been exceedingly painful and deserving of every consideration. The Bye-Laws have not allowed the Council to do anything except to proceed to the rigorous course of eventually expelling such Members. That has seemed, over and over again, very hard indeed. I have in my mind at this moment such cases, of men brought into pecuniary difficulties by agricultural distress, of clergymen who were perfectly unable to pay the arrears of their subscription, and by the Bye-Laws there was no possibility of the matter being remitted, or the Council exercising any power in their favour. In one of the Bye-Laws—I forget which—it is stated that should a Fellow be so many years in arrear with his subscription, the Treasurer should report his arrears to the Council, and that the Council have then no alternative whatever. They are bound, on the report of the Treasurer, to suspend the name of the defaulting Fellow, and eventually expel him. That, I am sure, Fellows will agree with me is a hardship which we should like to modify, and to put it in the power of the Council to exercise some discretion in the cases of numerous persons which are deserving of consideration. Those are the reasons which have induced the Council to suggest this alteration in the Bye-Law.

The President. I think I may put this at once to the vote, unless any Fellow desires to make comment.

The motion was carried unanimously.

The President. You have all received notice of a proposal by the Council to alter the Bye-Law as to the hour of meeting. I should explain how it comes about that this proposal is made by the Council. A Fellow of the Society sent in a notice in January which was exactly four weeks, less about three hours, before this present moment. Our Bye-Laws say that notice must be given one month before the Annual General Meeting. At its last meeting the Council did not care to pronounce what is the exact meaning of "a month," or whether the deficiency of an hour or two was material. I, for my part, express no opinion upon that. It appeared to the Council that the best way to proceed was to make this a motion of the Council, without expressing any opinion of its merits. The Bye-Law, altered as proposed, would read thus:—"The business shall commence at 5 o'clock in the afternoon precisely, when the Minutes of the preceding Meeting shall be read, and the Minutes of the Council laid on the table for the inspection of Fellows if required." That is the motion, but the Council, as I say, did not consider it on its merits. It is brought forward in order that the Fellow, who at least claims to have fulfilled the letter of the law, should not be ruled out on a technical objection. After these preliminary remarks, I invite discussion on this proposed alteration of our Bye-Laws as to our hour of meeting.

Mr. Knobel. I take it that the proposal of the Council does not require a proposer and seconder?

The President. I should imagine not, as in the other case which we have just considered.

The Astronomer Royal. I think if it is proposed by the Council that is sufficient.

Mr. Knobel. After the statement you have made, Sir, there seems to be one course which is most desirable. You state that the Council bring this forward without expressing any definite opinion upon it, and I infer from that that you mean it is brought forward rather for the purpose of elucidating the opinion of the Fellows than with the idea of legislating. Putting that interpretation upon your words, I beg to move the previous question. The effect of moving the previous question is that full discussion can take place upon the merits of the motion. If the previous question be carried, after debate, then the motion falls; but still you have the advantage of debate and discussion, and of hearing different views. If the previous question is negatived, then the substantive motion must be immediately put to the Meeting, and with that idea, if I am correctly interpreting what you meant, I make that motion, and proceed to make my remarks upon the question brought forward.

The President. I entirely agree that our meaning is as you have stated, but I have no power of binding the Society in any way. If they choose to vote for the alteration of the Bye-Law I am powerless to prevent it.

Mr. Knobel. This is an old question. It has been brought before the Society several times during the last ten or twelve years at least, and this is the fifth occasion on which this same motion has been made. On the four previous occasions it has been completely defeated. After every effort has been made to elucidate the views of the Fellows by plebiscite as to altering the time of our evening Meeting, the vote and the opinion of the Fellows at large have been invariably one way. They have been opposed to making any change. In my opinion that is quite correct. It is a question I am far from saying should not be investigated and enquired into, and views ascertained before we come to anything like legislation. We always notice that at the Annual Meetings we see different faces from those we see at the evening Meetings. That is a very significant point, for it shows that any vote taken by the Fellows assembled this afternoon would not necessarily be agreed to by those who attend our regular evening Meetings. We therefore should be extremely cautious how we proceed in altering our established customs. Such a custom can only be altered after a very careful ascertainment, not only of the opinion of the Fellows at large, but also only after a more careful investigation than has hitherto been made by the Council, as to how such a change would operate upon the conduct of business. It is obvious, when we are investigating a proposition such as that now submitted, that we should take into consideration the fact of the continuous meetings of Committees, Council, and Society; that the Fellows at large should not merely consider what is convenient for themselves in attending Meetings for general purposes, but should consider also what is convenient for the conduct of business at such Meetings as I have named. Supposing we were to meet at 5 o'clock, the Council would have to meet much earlier, and there would be no time between the Council Meeting and the 5 o'clock Meeting, and the Committee Meetings would have to be held in the morning. Then there is another point. It is largely an amateur Society. The majority of the Fellows are amateurs engaged in business, and therefore could not attend the Meetings in the mornings. It is an important point that such Committee Meetings, as would have inevitably to be held in the morning, might not be sufficiently attended by those whom we may desire to have upon the Council. Another point to be considered is the effect of the Council Meeting being held in the afternoon and being immediately succeeded by the afternoon Meeting. There will be the greatest chance of the business of the Council being hurried—there will be no latitude. Business may come on at the Council Meeting requiring long discussion, and it may not be completed, and therefore must be stopped. There will be a check, and the effect will be to shorten and curtail the proper consideration of business brought before the Council. For myself, I have always most strenuously opposed this change, because I think it will operate in a most pernicious manner to the interests of the Society. There is one feature also on which I have laid stress in past years, and I lay stress again upon it now. It is this, that the effect of such a change as this would be that, the moment the afternoon Meeting was over, everybody would rush away to dinner. Then we should be deprived of that feature of our Meetings here, namely, the assemblage in the library after the evening Meeting, which has struck me always as a most valuable privilege. We there meet our friends, we make acquaintances, we cement friendships, we are able to exchange ideas on our work, and discuss various topics. These will disappear altogether. That is a most important privilege which we should be slow indeed to deprive ourselves of. Those are, Mr. President, some of the views I desire to express most earnestly against this proposition, and which have induced me to make the motion I have.

A Fellow. I rise on a point of order. Does this motion require to be seconded?

Mr. Knobel. A motion for the previous question does not require a seconder. The equivalent to the "previous question" in the House of Commons is, that the question be now put, and that requires no seconder.

Mr. Mitchiner. This proposition to alter the time of the Meeting is either in the interests of the Royal Astronomical Society or it is otherwise. If it be in the interests of the Society I hope it will be carried. I believe it is in the interests of the Society, but if it can be shown by Mr. Knobel or anyone else that it is opposed to the best interests of the Society I shall be very pleased to see it defeated. On previous occasions, and in conjunction with Mr. Knobel, I have taken an active part in opposing this resolution, and the reason why I opposed it on previous occasions was because I knew that there were, at that time, certain members who were very regular in their attendance,—and I claim, perhaps, some authority about attendances; for I think Mr. Wesley will bear me out that during the last 10 or 15 years no one has been more regular at these Meetings than myself,—who would be cut off from the privileges of this Society if the time of the Meeting were altered, and that appeared to me a very undesirable thing. But the conditions are now entirely changed, and that is the reason why I sent the notice to the Council as to alteration of the hour of Meeting. I do not know of a single member now who would be excluded from these Meetings by the alteration of the time; but I do know of a great number of members, especially country members, who would be very glad to attend afternoon meetings. Mr. Knobel says we are an amateur Society. I think the argument from that fact has been thoroughly exploded. We have had an object-lesson in the formation of the British Astronomical Association, which, instead of beginning its Meetings, as was expected, at 8 o'clock, begin at 5 o'clock in the afternoon, and get full Meetings. At the same time, I know nothing about the convenience of the Council in the getting through their business, and I would be very sorry to advocate any change which would inconvenience that body; but I hope we shall hear an opinion upon this point from some member of the Council, for we do not want to vote anything that would be inconvenient to anyone, but we do want to go with the times. If it can be shown that this proposal is against the interests of the Fellows, by all means let us drop it.

Prof. H. H. Turner. Mr. Knobel has told us that on previous occasions he has always opposed this motion, and I am sure he will not object to me adding that he has somewhat vigorously opposed it. The last occasion on which it was brought forward was not long after Mr. Knobel had resigned the Secretaryship, a post which he held with the greatest ability, and for which the Society owes him a great deal. During the whole time he was of opinion the business of the Council could not be got through under a certain number of hours. As I am no longer Secretary—I held that position for 7 years—I may say that during my time we gradually found the business of the Council could be shortened, until the final result is that now the business takes one hour less than it did two years ago. For my part, I feel there is not the slightest question but that if the Society sees its way to adopt this motion, the business of the Council and Committee will be got through conveniently beforehand. I think I can claim to make that statement as a counterblast to Mr. Knobel's continued vigorous opposition to this motion. On the general question I do not pronounce any decided opinion. There may be many advantages either way; it is a question for the Fellows to settle themselves, but I am anxious that the consideration of the Council business will not occupy too large a share in the settlement of this point. This is the Annual Meeting, at which the Fellows can decide what the Bye-laws should be. Mr. Knobel seemed to imply that this was not a Meeting at which the question could be decided; but the Bye-laws tell us this is the time when such a motion should be discussed, and I am sure that if those interested in the matter cannot conveniently come in the afternoon, they might stretch a point and come to vote upon a question like this, if they thought it important. On the last occasion—six years ago, I think—the vote was adverse; but may be the experience since gained by the meetings of the B. A. A. may have changed the views of those who then defeated the motion. I have, however, some leaning towards the "previous question," for Mr. Knobel is going to be, I hope, our new President, and if he feels still vigorously opposed it, it might be delayed until he is no longer in office, for it would be ungracious to him to press the motion when he is about to enter the Chair. At the same time, if it does come to a vote, I shall be very glad to support the motion.

Mr. W. F. Stanley. I think there is something to be said in favour of a five o'clock Meeting. A very large number of the Fellows of this Society live out of London, and to attend the evening meetings they have to stop the night in town, possibly at some inconvenience. Another thing I should like to mention is that the Physical Society of London meet in Burlington House, in the adjoining rooms, and their Meetings are at 5 o'clock. The Council of that body meet at 4 o'clock, previous to the Meeting, or they may meet at 3.30. We find their meetings regularly and often very fully attended, and gentlemen of the Physical Society, I suppose, are about as free with their time as the gentlemen of the Royal Astronomical Society. The British Astronomical Association's meetings also point in the same direction. I think everyone should vote and should have an opportunity of showing in that way what is the most convenient hour for himself. Perhaps the Council might give the key to what would be the most convenient time to them for the evening Meetings of the Society; but for myself 5 o'clock would be the most convenient hour, indeed I cannot stay in London for the night.

The Astronomer Royal. I desire to add a few words to what Professor Turner has said. He has put the case very clearly, but there are one or two other points which I think ought to be prominently brought before the notice of the Fellows. I am sorry I differ altogether from Mr. Knobel on the question of the practicability of getting through the business of the Council and of the Committees before an afternoon meeting, and I think it is really a question for the Fellows themselves to settle, whether it is more convenient for them to meet, say at 5 o'clock in the afternoon or 8 o'clock at night. As regards the Council, under existing arrangements the Council is usually called at 4.30, and ends its business by 5.45, excepting on rare occasions. Now I put it to the Fellows, that if the Council can get through their business (as they have done lately by doing it in a more business-like way) in an hour and a quarter, it is not a serious thing to ask them to meet three quarters of an hour earlier than 4.30, so as to end their work by 5. I would propose instead of having the gathering in the Library after the Meeting, as we have now, that we should have this gathering with tea at 4.30, which is more in accordance with modern ideas. In the case of the Royal Society I am sure the Fellows of this Society will admit that that body has as much business to do as the Royal Astronomical Society, but they do it in an expeditious way. They understand that the time taken up in the transaction of the business of the Council must be limited, and they do not do as much talking in the working of their Council's affairs. The Council of the Royal Society meets usually at 2.30 or 3, and finishes its work by about 4. Then there is an interval for the Council and the other Fellows of the Society to meet and discuss various questions which interest them, a cup of tea being served meanwhile. Now, I imagine the same thing can be done in this Society. I think if we put the time of meeting half an hour later than the Royal Society, namely 5, we shall make a sufficient concession to those who take time over the business, and who object to coming early in the afternoon. In the Royal Society the practice is to have Committee meetings before the Council meeting—after lunch—sometimes at 2 o'clock, and sometimes at 2.30, but the Fellows manage to get through the business not only of the Council, but of the Committee, in time to have the afternoon meeting at 4.30. At the same time I feel that, if our President designate has a strong objection to it, we ought not to press this alteration upon him when he is taking office for this year. I do not wish to urge the proposal personally in any way, though I feel strongly its adoption will be for the interest of the Society. Still, I feel I must say that in my view the work of the Council can very well be done without inconvenience if the hour of meeting is changed to 5 o'clock.

Mr. Knobel. I must emphatically say, in reference to the remarks made by the Astronomer Royal, that no consideration for me must in any way influence the question whatever. If the Fellows decide, and the Council also decide, that the Resolution is for the good of the Society at large, I must not be considered in any way, and I shall disappear from being a hindrance to that which will benefit the Society.

Mr. M. Horner. I am rather surprised at the attitude Mr. Knobel has taken up, for directly I saw the motion I thought it came from him.

Mr. Knobel. I did not know anything about it.

Mr. Horner. I thought he might have desired the change that he might be able to attend the meetings of the Royal Astronomical Society and the Royal Institution, for the two meetings clash in a most unusual way. Mr. Christie has shown that the proceedings of the Council are not of a lengthy duration, which I have often thought to be the case.

Admiral Sir Erasmus Ommanney. I am certainly in accord with the views of the Astronomer Royal. I have been a frequent attendant here for the last 30 years. I am not idle during the day, I fill it in with various occupations; but I have a strong objection to coming here after dinner. In the Royal Society the change to the afternoon meeting has had the effect of doubling the number of members attending, and for myself I believe the afternoon tea at 4 o'clock is always a most agreeable opportunity for friends to exchange views. I therefore strongly support the change.

Mr. Newall. I should like to say a word or two in explanation of the circumstances under which the notice was given. It is, however, unnecessary to emphasize that more than by saying that the Council in bringing it forward merely wished to put no difficulty in the way of the Fellow who wanted to raise a question which affects the interest of the Society. Mr. Mitchiner, who gave notice, will, I feel assured, agree with what has fallen from Professor Turner and the Astronomer Royal, that, in spite of what Mr. Knobel has so gracefully said, it seems the most important thing we have to consider now is, that we should not carry the motion against the convenience of our President designate. He is probably to hold office during next year, and I feel rather strongly upon the point for that reason. My own opinion is that we should vote now upon the previous question and not on the actual merits of the case. I cannot, of course, say anything which would commit the Council, but I feel that I ought to be ready to make an undertaking to the Fellows of the Society, if it is desired, to bring the matter before the Council so that it may be brought before the Fellows at a later meeting. I hope that this course will recommend itself to Mr. Mitchiner and the other Fellows present. I may say that we have received one or two letters from Fellows living at a distance, and I ought to make known definitely that persons living in the North, who attend the evening meetings not infrequently, would find it very difficult to get up in time for afternoon meetings. There are also one or two letters that have been received showing that Fellows would welcome a change to the afternoon time rather than the evening time, and I think it would be decidedly desirable that the question be considered with more leisure, free discussion, and consideration later on. I would therefore support Mr. Knobel in this matter.

Captain Noble. I should like to say, if it were in consonance with our bye-laws, that it would be better to discuss this at a meeting called specially. We ought to have the Fellows who attend in the evening here in order that they may have a voice in the matter. There can be no doubt that there is a very considerable amount of opinion in favour of our adhering to the old time of meeting, but to many of us the afternoon would be particularly convenient. I, as a member of the Council, ought to have no opinion on the matter, and I do not express any, although I may say the alteration would suit me better. If the Council has to meet at 2 o'clock in the afternoon, and I am on the Council, I must be there by 2 o'clock. That is all the difference it would make to me.

Major Kingsley Foster. The fact of a copy of the resolution having been sent to every Fellow shows that we are expected to vote upon it, and if we called a Special Meeting it would be the same thing. If Fellows do not come to discuss it on this occasion they would not come and discuss it later on.

The President. The solution suggested by Mr. Knobel appears to be a very good one. The carrying of the previous question would not pledge the Society as to change of hour. I do think that, although the matter has been brought forward with notice, it does come somewhat as a surprise, and I further think that a Special Meeting for the discussion of this subject would be an efficient way, and a more satisfactory way, of getting the real feeling of the Society. I would add that we cannot accept Mr. Knobel's generous denial of his own importance. If he does, and we know he does, find that his other business in life would render it exceedingly difficult for him to be here, then I cannot but think that we are bound in mere courtesy to a man who has been so admirable a Treasurer, and who we are now proposing to elect as President, to accept his proposal of the previous question. This will not negative the proposition for change of the hour of Meeting, but will merely shelve it for some short time. Therefore, not speaking now as your President—for my Presidency has now but a short time to run—I would urge you to vote in favour of Mr. Knobel's previous question.

Mr. Mitchiner. I would rather support Mr. Knobel's proposition in the circumstances in which he is placed. I think it would be better not to press the resolution now, but to leave Mr. Knobel the pleasure of bringing it forward himself.

The President. We are bound to take a vote on the subject, but not upon the motion, and unless any other Fellow desires to address the Meeting I will call for a division.

A show of hands was taken.

The President. It is perfectly obvious the "ayes" have it, and therefore the previous question is carried and the motion is disposed of.

The Meeting then proceeded to the election of Officers for the ensuing year. Major Kingsley Foster and Mr. Cushing were appointed Scrutineers of the ballot-papers. The following was the result of the election:—

President: E. B. Knobel. *Vice-Presidents*: Sir R. S. Ball, M.A., Dr. A. A. Common, Prof. G. H. Darwin, Prof. H. H. Turner. *Treasurer*: W. H. Maw. *Secretaries*: F. W. Dyson, H. F. Newall. *Foreign Secretary*: Sir William Huggins. *Other Members of the Council*: W. H. M. Christie (Astronomer Royal), Dr. A. M. W. Downing, Dr. J. W. L. Glaisher, Capt. E. H. Hills, Frank McClean, Major P. A. MacMahon, Capt. William Noble, Dr. A. A. Rambaut, G. M. Seabroke, Dr. E. J. Spitta, W. G. Thackeray, E. T. Whittaker.

Mr. Knobel then took the Chair.

It was proposed by *Mr. Hollis*, and seconded by *Sir Erasmus Ommanney*, "That a vote of thanks be accorded to the President, Vice-Presidents, and Members of the Council, who now retire, for their services in promoting the objects of the Society."

A vote of thanks was accorded to the Auditors of the Accounts and the Scrutineers of the Ballot, on the motion of *Col. Waterhouse*.

The following gentleman was elected a Fellow of the Society:—

Wm. Henry Robinson, Offendene, Walsall, Staffordshire.

The following Candidate was proposed for election as a Fellow of the Society:—

Thos. C. Bush, Somerville, Wells Road, Bath (proposed by the Rev. D. H. Sparling).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The third ordinary Meeting of the Session was held on Wednesday, January 31st, at Sion College, *Mr. W. H. Maw*, President, in the Chair.

The names of eleven Candidates for membership were read and passed for suspension, and the election of seven new members was confirmed.

Mr. Holmes read a paper in which he discussed Prof. Schaeberle's investigations in regard to the defective definition of the Crossley 36-inch mirror. Mr. Holmes argued that Prof. Schaeberle's conception that the image of a star was built up of superposed images in many planes and of many sizes was a mistaken one. He held that the image of a star at the focus was not the result of such piling up, but was an interference phenomenon, the effect of the mirror taken as a whole, and not the effect of an assemblage of parts working more or less discordantly. The image of a star was not a composite image, not an accumulation of images, but was one only, and in one plane only. The discussions on the subject had treated the mirror as made up not of small mirrors, but as consisting of zones. But this did not affect the reasoning. They were told that with a ratio of aperture to focus of 1 to 14 no discernible defect occurs. If a 12-inch mirror of 80-inch focus was used the central $5\frac{1}{2}$ inches would give a practically perfect image, and there would result a spurious disc about 0″·8 in diameter. According to Prof. Schaeberle, a zone $\frac{1}{2}$ inch wide round this could make a larger image superposed on the original 0″·8, and so on to the outer 12-inch zone, which would produce the largest disc of all. Now every one of the zones produced a larger spurious disc than the central aperture, and therefore their superposition could not reduce the size of the disc produced by that central aperture, but must increase it, and therefore the larger the aperture the larger must be the star-disc. But it is well known that this is contrary to fact: the reasonable inference, therefore, was that the mirror acted as a whole in producing the focal image, and that the image was one only, and not a composite of superposed images. Mr. Holmes concluded that, notwithstanding the soundness of Prof. Schaeberle's mathematics, they must look elsewhere for the cause of the failure of large reflecting telescopes.

Mr. Seabroke said he had not had an opportunity of reading Prof. Schaeberle's paper, and therefore he hardly liked to say that he differed from that gentleman; but he thought that Mr. Holmes had taken entirely the right view of the matter.

Mr. Crommelin read some notes on the total solar eclipse of 1900, May 28, which were accompanied by two diagrams; the first showed the position of the more important reference-lines on the Sun's disc at the time of the eclipse, and the second showed the stars and planets in the neighbourhood. The Sun, he pointed out, would be in Taurus not far from Aldebaran. The 4th and 5th magnitude stars κ^1 and κ^2 Tauri would be within the corona. To the left of the Sun, Orion and Sirius; above, Auriga and Gemini; to the right, Perseus; while Mercury would be some 2° below and to the right of the Sun, and as its disc would be fully illuminated, and it would also be near perihelion, it would be quite a conspicuous object. In fact the possessor of a good instrument might do worse than devote totality to a critical examination of Mercury. Venus, at its greatest brilliancy in the middle of Gemini, would be visible with the naked eye before the beginning of the eclipse, and would be resplendent during totality. There would be an unusual number of bright stars round the Sun, and it would be interesting to see how many of them could be detected with the unaided eye. The zodiacal light might be visible during the expedition, though not to such advantage as it would be earlier in the year; but the probable clear skies and shorter twilight would be in its favour. Venus, Jupiter, and Saturn would be very favourably placed for observation. Sixty-four seconds of totality might be reckoned on at Algiers. The *Nautical Almanac* data gave sixty-seven seconds; but from the experience of 1898 it would probably be less than that.

Mr. Maunder said that in 1896 a number of papers were contributed to the *Journal* in the way of suggestions for observations that might be undertaken, and thinking it would be found useful to follow a similar course in regard to the approaching eclipse, he had sought the co-operation of several of those who went to Norway in 1896, or India in 1898, viz., Miss Bacon, Mr. Johnson, Mr. Gare, and Dr. Downing. Mr. Maunder said the observations of first importance which could be undertaken by those who had no large instruments at their disposal were first photographic, and next with the telescope. Photographs of the corona could be taken with fixed cameras, and therefore they need have neither equatorial or driving-clock. Instantaneous exposures might be given, and exposures up to one second, if they had the image of the corona on a scale not more than half an inch to the Sun's diameter, would be quite permissible, and would secure a very fairly dense image even to a considerable distance out.

Among miscellaneous observations that a photographer might make were a series of photographs of the landscape at regular intervals during the eclipse. Photographs of the brightness of the corona by simply exposing the plate to the light without a camera might also be taken. The possessors of small telescopes might do good work by sketching small isolated portions of the corona. It was quite useless to attempt to sketch the whole. The polar plumes and the prominences would probably be of the greatest value. There remained the shadow bands, which should be observed both in the horizontal and vertical planes, meteorological observations, and, as Mr. Crommelin had pointed out, the stars and planets in the neighbourhood of the Sun visible to the unaided eye should be noted.

Mr. Seabroke, *Mr. Wesley*, *Mr. Holmes*, *Mr. Crommelin*, and *Dr. Downing* spoke on this subject. The Meeting was then reminded of the proposed Eclipse Expedition to Spain and Algiers. The s.s. 'Tagus' had been provisionally engaged; but unless a sufficient number of applications were received the arrangements would fall through. It was therefore of the greatest importance that all who intended to go should engage berths at once.

Dr. Downing then read a paper on "Where the Day Changes," and *Mr. Goodacre* read a paper by *Mr. C. F. Smith* on the "Palus Nebularum Region of the Moon," each of which was followed by an interesting discussion. The Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, February 21st, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. F. C. Bayard, LL.M.*, President, in the Chair.

Mr. E. Mawley, F.R.H.S., read his report on the phenological observations for last year, in which he showed that the weather for the year ending November 1899 was chiefly remarkable for its high temperatures, scanty rainfall, and splendid record of sunshine. The winter and summer were singularly warm seasons, while the autumn was also warm, but during the three spring months rather low temperature prevailed. In the early part of the flowering season wild plants came into blossom in advance of their mean dates, but after March they were mostly late in coming into bloom. Taking the country as a whole, the best farm crop of the year was wheat, the yield of barley proved also good, while oats were slightly under average. The crops most affected by the dry weather were those of hay and turnips, the latter being in most districts exceptionally poor. The only part of the British Isles where the summer drought was not severely felt was in Ireland, throughout a great part of which there was abundant keep in the pastures during the whole summer. This year was a very bad one for fruit. The yield of apples, pears, plums, and strawberries varied greatly in different localities, but was in most of them much under average.

Dr. R. H. Scott, F.R.S., read a paper giving the results of the percolation experiments which have been carried on at Rothamsted by Sir J. B. Lawes and Sir J. H. Gilbert, from September 1870 to August 1899. Three gauges were used, with 20 inches, 40 inches, and 60 inches depth of soil respectively; the area of each gauge being one-thousandth of an acre. The amount of water collected at the depth of 40 inches is always in excess of that collected at 20 inches, and also of that collected at 60 inches. In the winter months more than half the amount of rain penetrates into the soil and is available for springs, while in summer this amount only reaches a quarter that of the rain.

Some Remarkable Spectroscopic Binaries.

THE disclosure of Capella in the character of a close pair was eminently unexpected. What next? it seems to suggest. Is any star secure in its singleness? Perhaps not very many. But an attempt to answer the question in detail would lead us far afield; and we have now to deal with particular cases, to the exclusion of general considerations.

The line-of-sight work executed—and admirably executed—at Potsdam ten years ago, stamped Capella as an exemplary solar star, irreproachably regular in its movements. The resources then at Professor Vogel's command were, indeed, inadequate to bring out certain significant variations from the Fraunhofer pattern now known to exist. The spectrum, in point of fact, is both oscillatory and hybrid; but only with the most powerful and perfect apparatus can these singularities be rendered manifest. This recondite discovery was, nevertheless, made in duplicate, and announced independently, by Professor Campbell and Mr. Newall (see the *Observatory* for February). It is, for several reasons, of especial interest.

Capella is one of the very few spectroscopic binaries at a determined distance from the Earth. The carefully revised parallax of 0″·081 assigned to it by Dr. Elkin in 1897 may be called authentic, its smallness notwithstanding. It is, however, nearly up to the average for the ten first-magnitude stars of the northern hemisphere. The corresponding light-journey is just forty years. Located thus remotely in space, our Sun (taking its photometric magnitude to be −26·5 *) would appear 185 times fainter than Capella. Hence the joint volume of the components, if they are equal globes of solar *intrinsic* brilliancy, can be no less, in round numbers, than one thousandfold that of the Sun. And their joint mass ought to be even proportionately greater, owing to the tremendous internal pressure which must prevail in such vast orbs. Nor are there symptoms of a high countervailing temperature, except, to a limited extent, in one member of the pair.

But when we come to consider their motions, we are met, at first sight, by a very different result. They travel with a relative speed of 36 English miles a second, in a period of 104 days; which gives for their orbit a radius of 51½ million miles, and implies for the system, on the hypothesis of a plane passing through the Earth, a mass 2¼ times the solar. The discrepancy with the value derived from photometric data is formidable, though not wholly irreconcilable. For we are free to select *any* plane for the revolutions of the conjoined stars. Antecedently, it is as likely to make an angle of 90° as one of 0° with the line of

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* Prof. Pickering's value of −25·5 for this datum would enhance the disparity 2·5 times. Mr. Gore, on the other hand, estimates it, on plausible grounds, at −27.

The line forming the south-eastern boundary meets the somewhat straight line running from the trapezium towards the east, forming the definite upper (southern) boundary of the great sinus or "fish's mouth." The lower (northern) boundary of the latter is rather less defined, and not prolonged so far towards the east; the fish's mouth is partly crossed by small bridges running southwards from its northern boundary. To the west and north of the trapezium the nebula runs off in irregular streams trending towards the north-west, most of the streams being concave towards the west and south. The western side of the dense portion of the nebula is quite undefined, except for a short distance from the apex of the right-angled triangle, where it is sharply cut off by a dark marking (light in our negative), which is visible even in photographs taken with long exposures. To indicate the scale of our illustration it may be mentioned that the distance from the apex of the triangle to the point where its south-eastern edge cuts the southern boundary of the fish's mouth is about 3½ minutes of arc.

Since the nebula in Orion was first seen by Cyzat, of Lucerne, in 1618, few objects in the heavens have been more constantly observed and assiduously drawn than this remarkable and complicated structure. But in comparing the many drawings that have been made, one is principally struck by their extraordinary divergence. It is only too obvious that the vast labour expended by so many observers has for the most part done scarcely anything to advance our knowledge of the form of the nebula. In his elaborate monograph, published in 1882, Professor Holden carefully collated and discussed all known descriptions and drawings of the central portion of the Orion nebula. It was independently discovered by Huygens in 1656, and his drawing in the *Systema Saturnium* is the first attempt to delineate this wonderful object. Huygens's drawing roughly gives the same extent of nebulosity as the picture we reproduce, on which most of his stars can be easily identified; but his representation of the nebula is so rude as to be scarcely recognizable. He shows very roughly the "fish's mouth," but he seems to represent the three conspicuous stars that are arranged in a horizontal line to the south-east of the trapezium as involved in the denser part of the nebula, while, as the photograph shows, they are really just outside it. The drawings of Picard (1673), Mairan (1731), and Long (1742) all follow Huygens in this error, but in Le Gentil's drawing of 1758 the relative positions of the bright part of the nebula and these three stars are very fairly indicated, though in other respects the drawing is incorrect. Thus he gives the right-angled triangle an angle of 130°. Of course differences of instruments will account for some of the divergences of the drawings, but it is evident that they are mostly due to want of skill on the part of the draughtsmen. As instruments improve, however, and more time and attention are given to the work, the delineations more nearly resemble the object, and are therefore more like each other. The extremely beautiful drawing of Bond gives the best picture of the general appearance of the nebula, but is much surpassed in wealth of detail by that of Lord Rosse. Each of these drawings was the result of years of labour, but as real evidence for possible changes one good photograph would be far more valuable. Indeed a careful study of the photographs with their infinite complexity of minute detail makes one feel no surprise that such an object has baffled the most skilful artists and the most zealous observers.

After a careful and minute discussion of all the observations and drawings, Professor Holden concludes that there have been no less than 10 possible instances of change in the central portion of the nebula. Three of these he considers *possible*, or *probable*, two *undoubted*, and one *certain*. Most of these supposed changes are in the brightness of different portions of the nebula: Holden only mentions one "possible" instance of change of form. We may consider the weight of the evidence from these changes, in the light supplied by the photographs. The first photograph of the nebula was taken in September 1880 by Dr. Henry Draper, but of this I know of no published reproduction. There is, however, a good photographic reproduction of Dr. Draper's plate taken in March 1882, with an exposure of 137 minutes. It shows, as might be expected, a considerably greater extent of nebulosity than the photograph we reproduce, but far less detail: the star-discs are of course enormously larger. The brighter portions of the nebula are much over-exposed and edges which are fairly sharp and definite in Dr. Roberts's short-exposure photograph are in Draper's plate blurred and ill-defined. It is obvious that even the apparent forms of the details may be considerably altered by different exposure: no nebulous mass has an absolutely sharp edge, so that a longer exposure will necessarily extend the limits of the mass, and may thus materially change its shape. It is therefore impossible to identify all the details on Dr. Roberts's and Dr. Draper's photographs, especially as so many of them are lost in the latter. But, as far as it is possible to identify them, there appears no evidence whatever of change in form or in the relative brightness of the different portions. Measurements have been made where possible, but the result is the same, and I am compelled to conclude that, as far as the evidence before us enables us to judge, there is no proof of change having occurred during the interval of fourteen years that have elapsed between the dates of the two photographs. If I am correct in this view, the conclusions of Prof. Holden appear less probable. And in view of the fact that the comparison, even of *photographs*, taken under different conditions is by no means easy, it seems likely that the supposed changes deduced by Holden are really due to differences of atmospheric and instrumental conditions, and, above all, to the inevitable errors of observation. The more the drawings are examined the more impossible does it appear to base upon them any arguments with regard to changes either in form or brightness. Micrometrical measures are of course satisfactory where the forms are simpler and the outlines definite, but in the case of such a nebula as that of Orion, where the outlines are vague and the detail most complex, it is from photographs only that we can hope for any results in the future.

W. H. Wesley.

A Comparison of Tycho Brahe's Meridian Observations of the Sun with Leverrier's Solar Tables.

These comparisons were undertaken for my own information, with no idea to publication. I offer the results to the Editors of the *Observatory* for whatever they may be worth.

Before proceeding to figures, perhaps it will interest some readers if I briefly describe Tycho Brahe's method of connecting the line of sight with the graduation of his instruments—quadrants, sextants, and armillæ.

Like all his predecessors and those who came in the hundred years after him, he was obliged to rely on the perfection of mechanical construction for the elimination of instrumental errors. At either end of the movable radius used for pointing was a square, of few inches side, in metal, perpendicular to the plane of the instrument, and also to the line of sight. These two squares (*pinnacidia*) had to be of exactly the same size, of true figure, perfectly flat, and with perfectly true edges. These edges were usually defined by the inner edges of slits in a somewhat larger piece of metal *.

The centre of the outer square had to be exactly over the centre about which the radius-bar turned—presumed to be identical with the centre of the graduated arc,—and the centre of the other square had to be exactly over the pointer by which the arc was read. In addition, the corresponding edges of the two squares had to be parallel to each other, and parallel and perpendicular to the plane of the instrument. Mechanical perfection was, of course, unobtainable; but immense care was devoted to this well understood object, and a high degree of perfection was attained. The zero of altitude was obtained by means of a plumb-line (often two, one on either side) suspended from the centre of the instrument, and made to correspond with the 90° of the arc.

Supposing the observation to be that of a star's meridian altitude, the quadrant was clamped with its plane—always truly plumb—in the meridian, and the observer moved the radius-bar until the star seemed to be in the plane defined by the upper edges of the two squares, then, lowering his eye, the star should also be seen in the plane of the two lower edges. As a star would require several minutes to traverse the field, that is, the length of the side of the square, the observer could leisurely verify. For a pointing in azimuth it is doubtful if both edges could be used, except on such stars as had slow azimuthal motion.

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* Engravings representing the Pinnacidia may be seen in the 'Mechanica,' the 'Historia,' and the 'Progym.'

For the Sun the mode of pointing was entirely different. The outer square was pierced with a small hole which would produce a more or less well-defined image of the Sun on the anterior surface of the inner square, on which surface was traced a circle (or several circles) concentric with the square. The observer had to judge when the Sun's image, with its ill-defined border, was exactly concentric with the circles. At the summer and winter solstices the surface receiving the Sun's image would be presented to the observer in a different manner with regard to the vertical, and it is just possible this may account for a portion of the discordances noticed lower down.

Tycho's great mural quadrant of 10 feet radius was a fixture—indeed it seems to have been built into the solid structure of the edifice. The fluctuations of its zero would be difficult of detection with the plumb-line. Tycho frequently records the reading to 5″, which shows his confidence in the graduation. This instrument commanded the southern half of the meridian, and the Sun was observed with it on almost every possible opportunity. Stars north of the zenith were observed with his large azimuthal quadrant (*Quad. Max. Chalyb.*), the *Quadrans Volubilis*, and others.

I now proceed to give the results of a reduction of a portion of Tycho's meridian observations made at Uraniburg in the years 1582 to 1590, given in 'Historia Cœlestis.'

LATITUDE.

In the year 1582, 115 meridian altitudes were observed north of the meridian, 214 south, of 83 stars.

The mean places of these stars for 1582·0 were formed from the mean places for 1755·0 in Auwers's 'Bradley' (except for Polaris) by means of the two precessions and proper motions there given.

For Polaris the following results were obtained:—

	Longitude.			Latitude.		
	°	′	″	°	′	″
1755·0 N.S.	85	8	24·06	+66	4	20·8
1582·0 N.S.	82	43	37·5	+66	2	58·5

	R.A.			Decl.		
	h	m	s	°	′	″
1582·0 N.S.	5	31	55	+87	2	25·5
By the method of Fabritius..	5	31	55	+87	2	24·5

to which proper motions -10^s and 0″ have been applied.

The reductions to apparent place were computed by the formulæ in Baily's 'Astronomical Tables.' The observed altitudes were corrected for Bessel's mean refractions. No data exist for zero corrections.

(1) *Results from Stars North of Zenith.*

	Latitude.	Obs.	Weight.		Latitude.	Obs.	Weight.
Polaris (ex Mer.)	55° 55′·3	12	2	α Cassiop. S.P.	55° 54′·0	1	1
Polaris S.P.	54·2	21	3	β Cassiop. S.P.	54·3	11	3
β Urs. Min.	53·9	8	2½	γ Cassiop. S.P.	55·4	3	1½
β Urs. Min. S.P.	53·9	7	2½	δ Cassiop. S.P.	55·2	7	2½
γ Urs. Min.	54·4	8	2½	κ Cassiop. S.P.	55·4	1	1
γ Urs. Min. S.P.	54·2	4	2	ε Cassiop. S.P.	55·1	8	2½
ε Urs. Min. S.P.	54·0	4	2	α Cephei S.P.	54·9	3	1½
β Draconis S.P.	53·8	2	1½	γ Cephei S.P.	54·0	4	1½
ζ Draconis S.P.	54·3	3	1½	ι Cephei S.P.	53·7	1	1
δ Draconis S.P.	55·3	2	1½	α Cygni S.P.	56·3	3	1½
ν Draconis S.P.	54·1	1	1	5 Urs. Min. S.P.	53·7	1	1

(2) *Stars South of Zenith.*

	Latitude.	Obs.	Weight.		Latitude.	Obs.	Weight.
α Arietis	55° 56′·2	2	1½	α Libræ	55° 54′·0	5	2½
β Cancri	52·9	1	1	β ,,	54·8	7	2½
α Can. Maj.	53·2	3	1½	γ ,,	54·8	1	1
α Can. Min.	53·7	1	1	ε ,,	55·1	1	1
α Ceti	54·4	4	2	15 Monoc.	53·6	1	1
β ,,	53·5	1	1	30 ,,	53·9	1	1
γ ,,	55·7	1	1	δ Ophiuchi	54·5	7	2½
β Erid.	54·5	1	1	κ ,,	55·4	5	2½
β Gem.	53·3	1	1	α Orionis	53 7	3	1½
γ ,,	54·6	1	1	β ,,	54·2	4	2
μ ,,	54·7	5	2½	γ ,,	54·1	2	1½
η ,,	53·6	6	2½	δ ,,	55·4	6	2½
ξ ,,	53·3	1	1	ε ,,	55·4	4	2
α Her.	54·5	2	1½	ζ ,,	53·0	2	1½
β ,,	55·2	6	2½	κ ,,	54·0	4	2
α Hydr.	54·3	5	2½	λ Peg.	54·6	1	1
θ ,,	53·4	1	1	μ ,,	56·0	2	1½
ζ ,,	53·4	1	1	β Scorp.	54·8	2	1½
α Leonis	54·9	9	3	δ ,,	54·5	4	2
β ,,	54·3	11	3	π ,,	53·6	4	2
γ ,,	54·8	8	3	α Tauri	53·8	6	2½
δ ,,	55·2	4	2	ζ ,,	53·3	1	1
ε ,,	52·7	2	1½	ξ ,,	55·4	1	1
θ ,,	53·9	2	1½	ο ,,	54·1	1	1
ι ,,	54·2	1	1	α Virg.	54·8	30	3
μ ,,	54·3	1	1	γ ,,	53·7	8	3
ο ,,	54·1	2	1½	δ ,,	54·9	2	1½
ρ ,,	54·2	2	1½	ε ,,	53·2	1	1
b ,,	54·5	3	1½	η ,,	54·2	4	2
54 ,,	54·9	2	1½	κ ,,	53·7	1	1
ι Leporis	52·6	1	1	φ ,,	55·1	1	1
κ ,,	52·9	1	1				

My idea in thus weighting the stars is to minimize the effects of errors of division, at the same time taking into consideration the number of observations of each star; no star, however, has a weight greater than 3.

The means are:—

	°	′	″
Of stars north of zenith	55	54	32
Of stars south of zenith	55	54	20
	55	54	26

Halley, in his 'Astronomical Tables,' gives the latitude of Uraniburg 55° 54′ 15″, and this value is frequently used by Tycho himself in the numerous little calculations of the obliquity, &c., he introduces between the records of observations proper.

One would naturally have expected that by subtracting 40″ for refraction, Tycho's latitude would have been improved; but it is not so.

It is possible to apply a correction to the above latitude founded on the known mean temperature and mean daily range of Copenhagen [*]. I have attempted to estimate this correction on the assumption that the year 1582 was normal [†]. 9 stars north and 6 south, culminating below 30° of altitude, indicated a correction of +2″ to the mean latitude.

G. L. Tupman.

[To be continued.]

Dr. Roberts' Photographs of Star-Clusters and Nebulæ [‡].

I am sure everyone who takes any interest in the science of astronomy will hail with delight the publication of volume ii. of Dr. Roberts's splendid series of stellar photographs. Since the publication of the first volume so much advance has been made in the photographic process of reproduction that all the plates are much finer in this than in the former series. It is difficult to find any fault in the printing of the 28 collotype plates which adorn this volume. On page 15 Dr. Roberts properly points out the great value of the permanent prints, by which the work of correlating the positions of the stars and nebulæ in the future may be carried out. The value of them will be enhanced if it is proved, as Dr. Roberts finds, that his negatives are not permanent, and that the images of faint stars in the lapse of a few years fade from the gelatine film. If this turns out, on further investigation, to be true of all gelatine photographs, it is most alarming, and would detract enormously from the value of our negatives in the future. It might be advisable to take a glass positive picture off the original negative as soon as possible, and then a second negative off this on dry collodion.

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[*] Oversigt......danske Videns. Salskabs, 1864.

[†] Meteorologiske Dagbog, Kjöbenhavn, 1876.

[‡] Volume II., *Knowledge* Office, 326 High Holborn, London, W.C.

Dr. Roberts mentions also the discoloration of the gelatine film after some time has elapsed. My experience is that this discoloration is due entirely to the plate not having been left sufficiently long in the bath of hyposulphite. It is often not sufficient to leave it in the bath until the high lights appear clear to the eye, but it should be left a considerable time longer, and then washed for some hours in running water. I have negatives that have been thus treated 20 years ago, and they are as clear and colourless as when taken; but others which were done in a hurry not long since are ruined by this yellow stain. Certainly this supposed fading ought to be thoroughly investigated, and if its reality is proved some means sought to overcome it.

Dr. Roberts next gives a most interesting chapter on the "Effects of Atmospheric Glare and of Diffraction upon the Films of Photographic Plates." By exposing plates in the bottom of a six-inch black box, which was open to the zenith, and covering the plate with a sensitometer figured scale, he found that the darkening of the plate due to atmospheric glare was about equal to the images of 16-magnitude stars taken with his 20-inch reflector, giving both the plate in the box and in the reflector a simultaneous exposure of 2½ hours; he gives this as a reason for spurious nebulosity, and then points out that the "general illumination of the atmosphere by starlight concentrated upon the film by the instrument will mask the light of objects that are fainter than about 18-magnitude stars. There then follow some examples of exposures ranging from 90 minutes to several hours on the Nebulæ in Andromeda, Orion, and field of stars in Cygnus, and finding in the long exposures no more stars than in the shorter ones, Dr. Roberts says :—"Here then is evidence . . . . . . which I think may be accepted as demonstration of the accuracy of the surmises of astronomers in the past, that the part of the Starry Universe visible from the Earth is limited in extent." Surely Dr. Roberts's researches point the other way? If we can photograph stars that are so faint that their images are only just discernible in the fog on the plate due to atmospheric glare, we can never expect to get any fainter stars on our plates, no matter how long the exposure is prolonged. The fact that we do get stars down to this limit is, I think, a strong argument that there are still fainter ones beyond our grasp and which theoretically must always be so. It would be a matter of great interest to compare photographs taken at, say, Arequipa or some station where the atmospheric conditions are nearly perfect, and those done in this poor climate of ours. The supposed grouping of the stars into lines and curves, the reality of which Dr. Roberts so strongly advances, will, I am sure, meet with a good deal of criticism. From the fact that on the plates there can be found groups of stars in lines and curves, which resemble the configurations of certain nebulæ, he puts forward an extension of Laplace's nebular hypothesis, namely, that stellar systems may have been evolved from nebulous matter. The following paragraphs are taken from his Introduction:—

> "I now propose to submit a series of photographic copies of my original negatives, from which we may obtain strong evidence if not complete demonstration of the evolution of stellar systems.
>
> "The appearances to which I now wish to draw special attention in the examination of these photographs are the numerous curves and lines of stars that are associated together in separate groups. The stars are of nearly equal magnitude, of approximately equal distances apart in each group, and the groups are independent of each other and of the surrounding stars.
>
> "These appearances are so numerous and persistent among the stars that their attribution to chance coincidences cannot be entertained, and we are irresistibly driven to accept as the cause that the stars in each group respectively are closely related to one another, and have been formed of similar material at a relatively co-equal epoch of time."

And later:—

> "If it is admitted that the evidence now produced proves, or even strongly indicates, the evolution of stars from the material of which spiral nebulæ are composed, from what source is the material derived? And is it gaseous or meteoritic? The existence in space of these different forms of matter is abundantly proved by the photographs of vast luminous clouds, and it is corroborated by observations of meteoric matter, and of cosmical dust falling continuously upon the Earth."

For myself, I must say that I think it most unlikely that these curves, which undoubtedly we can trace over all the photographs, have any cosmic meaning, as we must bear in mind the enormous distances which separate each individual star from its nearest neighbour, a distance probably equal to that of 61 Cygni from our Sun. If Dr. Roberts's views are in the future confirmed, it will add enormous value to these lovely photographs.

W. E. Wilson.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Interpolation and Raising Figures.

Gentlemen,

Allow me to express a mild surprise at the appearance of three of your notes at page 71 of your January issue, and inform you how we Americans do it.

We would not adopt any fictitious value of the constant of aberration, but make free use of second differences when we feel like it, especially in interpolating for right ascensions of stars, say, over 50° in declination, where the accuracy of observation would justify it. This is done by a simple mental process based on the principle of a corrected first difference. The corresponding formula where the actual differences are printed, as in the British *Nautical Almanac*, is

$$a_1 = a_0 + \tau\{d_1 + \tfrac{1}{2}(d_2 + \tau d_2)\};$$

where a_0 is the next preceding tabular value of the R.A., τ is the proportionate interval of time, d_1 is the first difference in the next preceding "compartment," and d_2 the second difference, opposite a_0. For the American Ephemeris the formula is simply

$$a_1 = a_0 + \tau\{d_1 + \tfrac{1}{2}\tau d_2\},$$

wherein appears the superiority of the arrangement adopted there, where d_1 is the first difference, printed opposite a_0, and d_2 is the following second difference. But with either ephemeris, in the great majority of cases, the corrected first difference can be determined by simple inspection.

Mr. Cooke is sadly behind the times in the simple matter of disposing of *fives* "out of bounds." In this country, from time immemorial, we have "evened things up," as he now suggests should be done.

If you Britons are to unite with us Americans in elevating the rest of the world astronomically as well as commercially and politically, allow me to submit respectfully that you should first "square up" in such little matters. A. S. FLINT.

Washburn Observatory, Madison, Wis.,
1900, Jan. 26.

P.S.—Excuse me for employing somewhat the same vein which you have adopted in the Notes from an Oxford Note-Book. I think it is a good idea to have some such place for the many good and amusing astronomical incidents and anecdotes, and I am sure we all enjoy them on the whole very much.—A. S. F.

[For the naive P.S. I am very much obliged to Mr. Flint; and I can assure him that we are sufficiently well acquainted with the American language not to misconstrue his letter. I have already put Mr. Cooke's position right last month. As regards the earlier part of the letter, it seems to me, firstly, that the *partial* use of second differences indicated by Mr. Flint is an error of judgment; the interpolations should be carried all through, or else he *is* adopting a fictitious value of the constant of aberration, as I have pointed out: secondly, that this extra work, albeit restricted to a simple mental process, might be avoided by the artifice I mentioned, which can do no possible harm.—O. N.-B.]

Comet III., 1759.

Gentlemen,—

There can be little doubt that, as Mr. Monck suggests, there is an error of date in the record of the observation of this comet on the 1st of January, 1760. It appears to have been first seen at Lisbon on the 7th of that month; several French astronomers observed it on the 8th; the first English observer of it (unless we accept Dunn's as really on the 1st) was Dr. Mason on the 9th, and it was also observed on that evening by Short, Michell, Munckley, and Day. The orbit was afterwards calculated by Lacaille, and the perihelion passage took place on Dec. 17, 1759, at the distance from the Sun of 0·966. The comet discovered by Messier on January 25, 1760, passed its perihelion on the 25th of November, 1759, and therefore reckons as 1759 II. (Halley's, at its first predicted appearance, was 1759 I.), whilst the one of which we are speaking was 1759 III. Although Short estimated its motion at only about two degrees in a day on January 9, it really was considerably greater, probably not much less than a degree in an hour, it being then nearly at its closest approach to the Earth, and (as Short expresses it) "very visible to the naked eye." Munckley states that two or three persons had seen the comet in London on the 8th (the day before he observed it), but "they were not astronomers enough to give any exact account, either of its place or appearance." On the night in question (January 9) it was a little westward of Orion, nearly over ν Eridani; and, as Mr. Monck remarks, had it really been observed on the 1st of that month, with the rapidity of its apparent motion, it could not then have been in Orion. It is probable, then, that for January 1 in the *Annual Register* (the account there is copied into the life of Dunn in the 'Dictionary of National Biography') we should read January 9. Struyck noticed the similarity between the orbit of this comet and that of the comet of 1664, but Pingré showed that they could not be identical. The last observation of Comet III. 1759 seems to have been made by Cassini de Thury on February 8 (the comet being then situated in the constellation Aries), so that the duration of its visibility was little more than four weeks.

Yours faithfully,

Blackheath, 1900, Feb. 9. W. T. Lynn.

OBSERVATORIES.

Yerkes.—The "First Annual Report" has been issued by Prof. Hale and is a model of clearness. We should prefer to reproduce the Report in its entirety, but space forbids. However, we will follow the original as closely as possible.

History, Buildings, and Equipment.—It so happened that in the summer of 1892, when an observatory was comtemplated, Prof. Hale heard that the firm of Mantois was in possession of two optical disks for a 40-inch objective. When this was brought to the notice of Mr. C. T. Yerkes he at once purchased them, requested the Clarks to proceed with a 40-inch objective, and placed the mounting of it in the care of Messrs. Warner and Swasey. After careful consideration it was decided to erect the observatory on the shores of Lake Geneva, 75 miles distant from Chicago. The building was commenced in 1895 and everything practically completed in 1897. It takes the form of a Latin cross:

The 40-inch being in a 90-feet dome	West end.
The meridian room	East end.
The 12-inch Kenwood refractor in a 26-feet dome.	North end.
The 24-inch reflector (under construction) in 30-feet dome	South end.

The body of the building, between the domes, contains laboratories for physical and chemical work, offices for staff and library, On the ground-floor is an instrument shop, optical laboratory, photographic dark rooms, enlarging room specially fitted up for photos of stellar spectra, spectroscopic room, &c. &c.

The 53 acres of land was the gift of Mr. John Johnston, the building and equipments, including the 40-inch, its dome and floor. the 30-feet domes and stellar spectrograph, were the gift of Mr. Yerkes, while Miss C. W. Bruce presents a 10-inch photographic telescope, mounting, and dome.

Study and Research.—Students who have completed their work at the University may attend lectures on Solar Physics, Celestial Photometry, and Astronomical Spectroscopy by Professors Hale and Frost. And as the Observatory has facilities for special research, opportunities are given to men of science to carry on special investigations. During the past summer Dr. Nichols, Prof. of Physics in Dartmouth College, took advantage of this permission to study a stellar-heat radiator. The rays from Arcturus and Vega were reflected from the large silvered glass heliostat mirror to a 24-inch silvered mirror of 8-inch focus; these rays were focussed on the small mica vane of a delicate radiometer, the sensitiveness of which exhibited a deflection of 0·1 mm., corresponding to the heat that would be received from a candle 15 miles away. The mean deflection from 106 measures on seven nights was from Arcturus 0·60 mm., from Vega 0·27 mm. Roughly Arcturus apparently sends no more heat than a candle 6 miles away.

Astronomical Investigations.—Prof. Burnham has devoted two nights each week to the measurement of double stars with the 40-inch, the object being to secure measures of his own pairs for inclusion in a catalogue to be issued in the coming year. Prof. Barnard has also measured some double stars. κ Pegasi has been measured with a power of 3750. A faint companion to Vega was discovered by Prof. Barnard in May 1897, a few days after the instrument was erected. Prof. Barnard has also given special attention to certain variables in the clusters M 5 and M 13, and has devoted some time to planetary work. "The markings on Venus and Mercury have been carefully examined, but only the vague outlines, previously seen by Prof. Barnard at Mt. Hamilton, could be made out. A remarkable series of measures of the satellite of Neptune has been secured."

Spectroscopic Work.—Prof. Hale made a study of the spectra of stars of Secchi's fourth type and secured photos of 22 stars ranging in mag. from 5·4 to 7·8, with a stellar spectrograph attached to the 40-inch. Over 100 lines have been measured in the yellow and green regions of 152 Schjellerup, where not more than 3 or 4 had been previously recorded. It is believed that the spectra of these stars contain bright lines. Much other information with respect to these stars has been gathered.

A series of photographs of the spectrum of *o* Ceti extending through the maximum of 1897 has been taken by Prof. Hale and Mr. Ellerman. Prof. Wadsworth has photographed the spectra of several double stars and spectroscopic binaries. The 12-inch was used by Mr. J. A. Parkhurst for observation of variable stars.

Determinations of Latitude and Longitude give

Approx. Lat. of Yerkes Observatory .... $+42^\circ\ 34'\ 15'' \pm$
Approx. Long. " " .... $5^h\ 54^m\ 14^s \pm$ W.

HARVARD.—The Fifty-fourth Annual Report of the Director of this Observatory for the year ending 1899, September 30, opens with a statement of the financial position of the Observatory, a subject which was referred to in detail in this magazine for December last. Beyond this the Report is similar to those of former years, and gives details of the work done with each instrument.

East and West Equatorials.—These have been used as before for photometric observations of variable stars, of Jupiter's satellites during eclipse, of which 24 have been observed, making the total number 684, and for miscellaneous observations, such as occultations of stars by the Moon.

The Meridian Circle.—Observations with this instrument have been almost entirely limited to determination of clock-error. Experiments were made to determine whether the part of the field in which the collimation was determined had any influence on the result. It appeared that this influence was very slight, if it existed at all. The chief meridian work of the observatory is now the completion of the Catalogue for the zone -9° to -14°, and this is proceeding.

The 4-inch meridian photometer has been sent to Arequipa, Peru, where it has been used to make photometric observations of all stars south of declination -30°, and of the magnitude 7° and brighter, which will form a second series of observations of the stars in the Southern Harvard Photometry. At Harvard a 12-inch meridian photometer is now in use, and all the stars in the Bonn Durchmusterung from declinations $-20°$ to $+80°$ are being photometrically measured.

The discoveries made from the examination of the plates of spectra taken with the 11-inch Draper telescope include a new star in Sagittarius, the fact that ζ Centauri and π Scorpii are spectroscopic binaries, and several new variables. The most interesting of these last is B.D. $+42°\cdot3338$, which has a period of $13^h\ 35^m$, and whose light increases at the rate of about eight-tenths of a magnitude an hour, a rate surpassed by only two or three variables of the Algol type.

In the section relating to the Bruce Photographic Telescope, reference is made to the discovery of the ninth satellite of Saturn, by Prof. W. H. Pickering. The interest of this subject induces us to quote verbatim: "This object, to which the name of Phœbe has been given, appears on four plates taken on August 16, 17, and 18, 1898. The position of all these images is closely represented by assuming that its period is 490 days. Owing to the extreme faintness of this object it can be photographed only when Saturn is near one of its stationary points. The plates taken in September 1898 were not sufficiently good, and clouds prevented good results being obtained in the spring of 1899. It is hoped that photographs taken in August 1899 will enable the orbit to be accurately determined."

Odessa.—In response to a representation made by Herr O. Backlund in 1895, the Governing Body of Poulkova Observatory have established a branch observatory in Odessa. The building was begun in the summer of 1897, and in August 1898 the transit circle was installed. The scheme of the work is based on that of Poulkova, and a list of 170 stars down to 6·4 mag. is given in the Report. The buildings consist of a transit hut and a mark house 119 metres to the north.

The transit has a Steinheil objective of 108 mm. aperture, and 1·30 m. focal length. The hut is so constructed that while the central portion remains fixed, the east and west ends can be run away on tramways and so leave two openings, one for the transit, the other for a vertical circle with a similar objective, 1·40 m. focal length. The present Report emanates from Herr A. Orbinski, who is in charge. Photographs of the buildings and the instruments are given.

PUBLICATIONS.

Nice Annals, Vol. I.—We made mention of the publication of Volume IV. of the Annals of this Observatory in 1895, and

Vol. V. has since appeared, so that there may seem some want of continuity involved in this heading; but, as is not unusual, the first volume of the series has been reserved for a description of the foundation and building of the observatory and of the instruments, and is therefore not published until the completion of the establishment, when work may have been for long in full swing.

It may not be generally known that the magnificent Observatory at Nice owes its being to the generosity of M. Bischoffsheim, who, desiring to raise a lasting and worthy monument to French science, more than twenty years ago approached the Bureau des Longitudes with the offer of a sum of money to create an observatory. Very naturally the Bureau accepted the offer, and in 1879 appointed Commissioners to choose a site and select instruments; and as a result there is to-day a finely-equipped observatory on Mount Gros, Nice, where actual work is in progress under the direction of M. Perrotin, who has a staff of eight Assistants, the establishment being finally in the hands of the Paris Bureau des Longitudes.

The instruments of the observatory comprise the well-known 30-inch refractor; an equatorial with telescope of 15 inches aperture, and an equatorial *coudé*; also a transit-circle of 20 cm. aperture and 3·20 m. focal length, and a smaller transit-circle; this is reversible, with an object-glass of 7 cm. aperture. These instruments are described with much detail in the volume before us, which is accompanied by a series of plates showing their construction. Of the many interesting things in these descriptions we select the account of the meridian marks of the larger transit-circle for special mention. This instrument is furnished with two meridian marks, one north and one south of the instrument. The south mark is merely an ordinary collimator set up in the observatory grounds, and lit up at night by an electric lamp; but the north mark is situated on Mount Macaron, 6½ kilometres from the observatory a deep valley running between Mount Macaron and Mount Gros. As there would obviously be some difficulty in carrying wires from the observatory to light the mark, a plan proposed by M. Fizeau has been adopted. The mark is a reflecting collimator, that is an object-glass with a plane mirror placed behind it at its focus. In the observatory on either side of the transit-circle, each at a distance of 35 cm. from the meridian, are two small telescopes (16 cm. aperture, and 1 metre focal length), which have illuminating lamps in their focus, and are directed to Mt. Macaron. They thus send to the collimator two beams of light, which are reflected, and of which the greater part returns to the point of origin, but on leaving the collimator the beams expand by diffraction in all directions. That portion of the diffracted light which returns along the line bisecting the angle between the two beams is received by the transit-circle and forms an image of the mark. The arrangement works quite satisfactorily. The description of the instruments is supplemented by various Tables used in reduction of the observations made with the instruments; some of which are useful only to this observatory, but others, such as the precession tables, are of wider interest.

NOTES.

COMET NOTES.—A very faint comet of the 13th magnitude (α 1900) was discovered by M. Giacobini at Nice on January 31.

The following orbit is deduced from observations made there on Jan. 31, Feb. 3 and 6. An observation made there on Feb. 17 is well represented by these elements:—

T ……	1900 April 28·2085, Paris M.T.	
ω ……	$23^\circ\ 8'\ 42''$	1900·0.
☊ ……	40 7 29	
i ……	146 37 22	
$\log q$ ….	0·12902	

Ephemeris for Greenwich midnight, computed by Dr. Smart.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Mar. 2….	2 5 42	2 55	Mar. 18…	1 53 10	7 22
6….	2 1 59	4 5	22….	1 50 43	8 24
10….	1 58 44	5 12	26….	1 48 35	9 24
14….	1 55 47	6 18	30….	1 46 33	10 24

The brightness will remain pretty constant for several months. In April the comet will be lost in the sunshine, but it will reappear a month or two later, and next July will still be as bright as at discovery.

The comet was observed by M. Javelle at Nice on Feb. 17, being 7^s west and 1′ north of the position computed from the above elements.

The number of observations of Tempel's Second Periodic Comet made at the recent apparition is very large. *Ast. Nach.* 3618 contains a series of observations made at Cordoba (Argentine Republic) by Mr. James Renton, the dates ranging from 1899 July 11 to Oct. 11. We learn from *Ast. Journ.* No. 477, that the comet was followed at the Lick Observatory till Dec. 1 last, when it was described as a very faint diffused object of the 15th magnitude, about 1′ in diameter, with scarcely any condensation. When brightest the comet had a well-defined stellar nucleus, so the observations should be very accurate, and it is hoped that they will afford material for a good determination of the mass of Jupiter. A. C. D. C.

MINOR PLANET NOTES.—*Ast. Nach.* 3620–21 contains tables for the computation of the perturbations of those planets whose period approximates to half that of Jupiter, by H. v. Zeipel.

The quantities are arranged in powers of an auxiliary quantity w, which $= 1 - \frac{\text{Twice Jupiter's Mean Motion}}{\text{Planet's Mean Motion.}}$.

The perturbations of the first order of (48) Doris are worked out as an example.

Ast. Journ. 477 contains an analysis by Prof. Newcomb of the grouping of the orbits of planets (1) to (400) according to their mean motions. Only those whose mean daily motions lie between 500″ and 1000″ are considered, and these are arranged in groups of 10″, the first containing mean motions between 500″ and 510″, the next between 510″ and 520″, and so on. The numbers of planets falling in each group are as follows :—

500″ to 600″	0,	0,	0,	0,	2,	3,	4,	1,	0,	0
600 to 700	0,	6,	9,	19,	27,	8,	10,	5,	12,	3
700 to 800	2,	8,	11,	10,	1,	9,	22,	25,	18,	11
800 to 900	10,	16,	9,	10,	7,	11,	5,	3,	2,	0
900 to 1000	1,	6,	6,	9,	8,	6,	11,	9,	5,	4

There are blanks in the neighbourhood of 600″, 700″, 745″, 900″, which correspond to mean motions whose ratios to that of Jupiter are $\frac{2}{1}$, $\frac{7}{3}$, $\frac{5}{2}$, $\frac{3}{1}$. There can be no reasonable doubt that these gaps are in some way the effect of Jupiter's action. On the other hand, there are two planets whose mean motions are almost exactly 449″, which corresponds to $\frac{3}{2}$ of Jupiter's, so that this ratio does not seem to be an influential one in causing a gap.

It will be seen that the richest regions of the zone are at mean motions 640″ and 775″.

The interesting planet (434) Hungaria will be in opposition about March 23, being of the 12th magnitude. *Ast. Nach.* 3624 contains an ephemeris.

A new planet FA, of the 12th magnitude, was discovered by M. Charlois at Nice, on Feb. 22. A. C. D. C.

OBITUARY.—CHARLES PIAZZI SMYTH. The death-roll of the past month includes the name of the former Astronomer Royal for Scotland, a well-known figure in astronomy some forty years ago, but probably little known except by name to the present generation.

Charles Piazzi Smyth, born in the year 1819, was the second son of Admiral Smyth, the author of the 'Celestial Cycle' and sometime President of the Royal Astronomical Society. His second name was given him in memory of the famous astronomer, the discoverer of the first of the small planets, to whom the family was related, so that Charles Smyth may be considered an hereditary astronomer. In the year 1835 he was appointed Assistant to Maclean at the Royal Observatory, Cape of Good Hope, which post he held for about ten years, and in the year 1845 he was chosen to succeed Thomas Henderson as Director of the Edinburgh Observatory, a post which carries with it the title of Astronomer Royal for Scotland. In March 1846 he was elected a Fellow of the Royal Astronomical Society, and made many contributions to the Society's literature.

In May 1856 he was commissioned by the Admiralty to proceed to Teneriffe to make experiments to ascertain how far observations can be improved by eliminating the lower strata of the atmosphere. A sum of £500 was placed at his disposal for this, and the yacht 'Titania' was lent him by Robert Stephenson, M.P. He returned in October of the same year, and published a history of his journey and observations in a book *, and his scientific results in the *Philosophical Transactions* †. One conclusion that he arrived at was that at a height of 8903 feet above sea-level there was "an extension of telescopic vision through four stellar magnitudes."

Meteorology evidently had great attractions for Piazzi Smyth, for several years later, in 1882, he made a stay in Madeira, and on his return published a small book entitled 'Madeira Meteorologic,' and also an account of spectroscopic observations made at the same place. The subject of spectroscopy was also a favourite study, and in this he did valuable work. Smyth was one of the first to resolve the spectrum into a rhythmical series. He was the author of a book entitled 'The Solar Spectrum in 1884.'

But his name will doubtless be chiefly remembered in connection with his researches about the Great Pyramid. In 1864–1865 he went to Egypt accompanied by his wife, who was with him also on the occasions above mentioned, and spent four months at the Great Pyramid, living in an empty ancient tomb carved in the side of a hill. During this time he made a most exhaustive series of measurements of the great monument, which form the data for his book, 'Our Inheritance in the Great Pyramid.' This book has been the subject of considerable controversy. It states that the Great Pyramid is of Hebrew origin, and was made about the year 2170 B.C., which follows from the fact that the descending entrance passage then pointed to α Draconis at its lower culmination, and it has been suggested that the ascending passage branching from this was used to observe the transit of the same star by reflection. Piazzi Smyth was of opinion that not only did the pyramid record the fundamental truths of astronomy and physics, but its dimensions and details contained cryptically the units of the Biblical system of measurements, and that it was also of a

* 'Teneriffe: an Astronomer's Experiment.'

† Vol. cxlviii. p. 465 (1858).

prophetic nature, the date of the birth of Christ being indicated by certain features of the architecture.

Smyth gave up his position at Edinburgh in the year 1888, since which time he has lived a life of retirement at Ripon. His wife survives him. One of his sisters married Prof. Baden-Powell, of Oxford, so that he was the uncle of the gallant Major Baden-Powell, now besieged in Mafeking.

Dr. Robert Luther. We regret also to announce that Prof. Luther, Director of the Dusseldorf Observatory, died on Feb. 19, at the age of 78. In early life he worked at the Berlin Observatory, and computed ephemerides of asteroids for the 'Berliner Jahrbuch,' and in 1851 was called to succeed Brünnow as Director of the Observatory belonging to the town council of Dusseldorf. Here, with a comparatively small telescope of 4·3 inches aperture, he devoted himself to the discovery of small planets and computation of their orbits, and of the 460 at present known 24 have his name attached as discoverer. Dr. Luther contributed many observations and ephemerides to the *Astronomische Nachrichten.* We had no actual personal knowledge of the late astronomer, but we shall miss the pleasant little notes with which he occasionally favoured us.

The Coming Solar Eclipse of May 28.—We are beginning to hear of the arrangements of expeditions to observe this eclipse. The French Astronomical Society have had a steamship placed at their disposal, and both it and the British Astronomical Association will send parties to observe from Alicante and Algiers. Mr. Percival Lowell and Prof. Todd have already left the United States for some suitable station in North Africa. Messrs. Cook & Son have arranged a conducted tour to Talavera, starting from London on May 21.

Double Stars.—Recent measures of β 883 appear to point to a period more nearly approaching that deduced by Glasenapp (16·8 years) than to the short period (5½ years) suggested by Dr. See.

	°	″	
1898·43	39·3	0·27	Aitken (Lick).
53	42·9	0·33	Lewis (Greenwich).
74	45·3	0·19	Barnard (Yerkes).
1899·53	53·2	0·32	Lewis.
75	54·4	0·25	Aitken.
78	54·0	0·22	Barnard.
1900·05	61·0	0·28	Lewis.

The latest measures of Sirius with which we are acquainted are by Prof. E. E. Barnard:—

1899·830 .... 151°·3 4″·83 2 nights.

The same observer gives places of several small stars near Sirius, whose distance from the principal star it would be interesting to measure. The proper motion of Sirius being 1″·31 annually, a few months ought to show whether these small stars belong to the system. It can be seen that E and F do not.

		°	″	
AE	1897·84	195·5	57·69	
	1898·74	194·0	56·80	E=16 mag.
	1899·88	193·7	54·70	
AF	1897·84	3·0	50·08	
	1898·85	4·6	53·12	F=16½ mag.
	1899·86	....	53·10	
AH	1899·86	280·6	40·25	H=16 mag.
AK	1899·86	22·4	29·53	K=16 mag.
AG	1899·17	249·0	86·78	

VARIABLE VELOCITY IN LINE OF SIGHT OF δ ORIONIS.—M. Henri Deslandres is energetically using the large photographic telescope (·62 cm. aperture) of the Meudon Observatory to take photographs of the spectra of stars, for determining radial velocities. Since Oct. 15 last he has accumulated 60 such photographs, and he gives results derived from eleven plates of δ Orionis which are of interest (*Comptes Rendus*, 1900, Feb 12).

δ Orionis was noted by Herschel as variable, but of irregular period. Vogel and Scheiner measured its radial velocity at four different epochs, but did not find differences greater than 7 km. per second. The following table gives M. Deslandres' measures of velocity of the star, which he says are not to be taken as perfect, but nevertheless are good enough to give credibility to his conclusions:—

	km. per sec.		km. per sec.
1899 Dec. 8	+95	1900 Jan. 9	−37
9	−15	10	+64
12	+70	12	+80
15	−38	13	−49
18	+81	18	+14
		25	−50

Owing to the difficulty of the observations these figures may be sometimes as much as 15 km. in error; but M. Deslandres says, in conclusion, that they accord well with a period of 1·92 days and a large eccentricity of orbit.

PROF. TURNER'S LECTURES.—As was announced, Prof. Turner gave a series of three lectures at the Royal Institution, in the afternoons of February 8, 15, and 22, on Modern Astronomy. The lectures deserved the title, for they certainly were "modern" and were not mere exhibitions of pictures that all are tired of seeing, and their delivery was in Prof. Turner's most incisive manner. He gave a sketch of most branches of modern astronomy in the three hours at disposal, which, as the lecturer said, was barely long enough. For similar reason we can scarcely sketch the series here, but content ourselves with giving categorically some of the points which seemed most striking to some of the audience:—(1) An effective illustration of the phenomena of Jupiter's satellites with a globe, a beam of light, and a golf-ball. (2) A dramatic representation (with the actual instrument used in 1898) of the operations during totality of a solar eclipse. (3) Illustration with a "bull-roarer" (an instrument which hums when whirled at the end of a string and emitted a different note as it moved towards or from the audience) of the principle of spectroscopic observation of the motion of stars in the line of sight. (4) The pictures of nebulæ and their comparison with that of Saturn, in illustration of the nebular hypothesis. (5) The charts showing the relation of the Wolf-Rayet stars and star-clusters to the Milky Way, and the hinted suggestion by the lecturer that the Universe itself may be a rotating sphere, of which the galaxy is the Equator. To this statement of a few facts it may be added that the lantern was operated and the illustrations performed with the precision that characterizes lectures at the Royal Institution.

WE call attention to the occultation of Neptune by the Moon on March 8 at 6 o'clock in the evening, and to that of Saturn at half-past 8 on the morning of March 24 (civil day). On March $27^d\ 4^h\ 30^m$ P.M. Venus approaches within 2′ of δ Arietis.

PROF. BURNHAM'S Catalogue of Burnham Stars is completed, and some copies have already reached England. We propose to give an adequate notice of this in our next number.

THE next Meeting of the Royal Astronomical Society will be on March 9, at 8 o'clock in the evening. Of the British Astronomical Association on March 28.

FROM AN OXFORD NOTE-BOOK.

RECKONING by civil time there was no new Moon last month, the change of phase happening just before noon on March 1.

A similar occurrence with regard to full Moon is thus chronicled by a weekly contemporary :—

A Remarkable Month.

> February, 1866, was in one way the most wonderful month in the world's history. It had no full Moon. January had had two full Moons, and so had March; but February had none. This remarkable state of things had never happened before since the Creation.

The value of the store of photographs at Harvard Observatory as records of the state of the sky has several times been demonstrated, as, for instance, in the recovery of positions of Eros in 1894 and 1896, after its discovery in 1898. I recently had another opportunity of testing the completeness of the information, and the response to the test was so satisfactory that it may interest others to hear of it. The star B.D. +27°·2561 was suspected of variability, not being found on one of our Catalogue plates at Oxford, and being faint on another. I wrote to Prof. Pickering asking if he could give me any information about it, and he promptly replied :—

> I have had a number of photographs of +27°·2561 examined. These plates were taken with the 8-inch Draper Doublet on March 21, 1890; May 25, 1891; May 31, June 12, July 7, July 8, 1892; June 30, 1893; March 23, June 13, 1894; February 26, 1896; June 25, 1897; June 17, 1898; and August 18, 1899. The exposure was generally 10 min. On all it appears faint, but of so nearly the same brightness that we have failed to detect any variation. Probably the star is red, and therefore faint photographically.

Here was a question about a chance star, and it was found on 13 plates extending over 10 years! Surely this kind of information, which is the outcome of many rapid and successive surveys of the whole sky, is more what we want than a single laborious survey extending over half a century? There is room, of course, for both kinds of survey, but I cannot help feeling that the proposals for the work of the Astrographic Chart would be the better for modification in the direction here indicated. We ought to finish it in a reasonable time and then repeat the work. And it may be remarked that there is already some tendency in this direction. Dr. Gill, for instance, announces that although he has taken all his Catalogue plates, he is taking them again, so that he may be able to check proper motions. An excellent plan.

The printing of the R.A.S. Council Report generally produces a crop of whimsical printers' errors, and the present year was no exception to the rule. These all disappear under the careful scrutiny of the Secretaries and other members of Council, but some of them are worthy of preservation for the delectation of those who do not see the report in proof. The two most amusing referred this year to comets. Of one it was remarked that "The *artist* is probably parabolic," and of another, "Since the distance from the Earth was also increasing the comet remained a *public* object." An ambiguous phrase in the report of an observatory, "We have also watched for lunar eclipses," makes one wonder whether the watchers had any better luck than those who tried to trap meteors by exposing plates "from the time of rising of the *radical* until dawn." Being now no longer responsible editorially for any of these slips which may by accident get overlooked, I can afford to call attention to some of them with a light heart; but I have had my agonized moments of finding such escape the most careful supervision. There is in a number of this magazine for which I was partly responsible the following curious phrase, for which I do not feel called upon to give the exact reference, or the name which may identify it—"Comparatively small telescopes" (as Mr. —— *contemptibly* says); and again there is in a number of the *Monthly Notices* a paper with a startling dual authorship—due to the misplacing of a comma (perhaps I should state that certain Studentships at Cambridge are called Isaac Newton Studentships). The author(s) is (are) thus styled: X. Y. Z., Fellow of Trinity College, and Isaac Newton, Student in the University of Cambridge.

THERE was a very successful gathering at dinner after the presentation of the R. A. S. Gold Medal to M. Poincaré; but the distinguished Medallist could not be induced to make any considerable speech, either in English or French. We were glad to see present Capt. Sparkes, R.N., who did so much for the abortive eclipse expedition to Japan in 1896, and fortunately Col. Rivers was also present to meet him, and told another of his good stories. He deprecated the idea that our military officers are averse to learning, by experience or otherwise, and instanced the manner in which they had at once given up their distinguishing badges when they were taught by the Boers that these marked them as rifle-targets. Some of them had learnt this lesson with such good will that they not only copied the clothing of their men, but begrimed themselves in order to further the resemblance; and the gallant Colonel assured us that one officer had been so enthusiastic in this direction that one of his own men had mistaken him for an ant-heap and dug a spade into him! In connection with matters which these stories recall, I will venture to return the thanks of the Junior Member of the 1896 Expedition to the amiable correspondent who remembered him last month. I fear neither he nor Col. Rivers will be going to Algiers with me next May. There is in the weekly periodical called *To-Day* for February 22 a would-be humorous, but I think on the whole rather ill-natured, commentary on the fact that we are to have this year an eclipse visible in England (*sic*!) and the United States.

Q.: Explain the statement that the Sun never sets on the British Empire.

A.: The Sun sets in the west; now the British Empire lies in the north, south, and east.

A.: What is the derivation of the name *theodolite*?

B.: Oh! I suppose it comes from *θεωρέω*, *ὁδὸς*, and *λίθος*, to observe stones in the road, doesn't it?

A.: Oh, no, it doesn't. It is called after a Frenchman named Theodolite, who invented it.

Turning over some old letters recently, I came across the following in a letter written by the late J. R. Hind in 1875 to the present Director of the Liverpool Observatory. I hope there is no impropriety in publishing it after the lapse of a quarter of a century:—

> I encountered Mr. Stone (who appears to be located at Staines) in Waterloo Road the other day, and, getting into conversation with him on Venus transit, Moon's acceleration, Southern star-places, &c., was equally surprised with himself to find that we had stood in the middle of the pavement for $1^h\ 55^m$!

I hear that Dr. Gill has been awarded the Watson Medal of the Washington Academy, and that other honours may be in store for him. His many friends will rejoice greatly, and await further details with interest.

A furious attack on the Report of the Board of Visitors of the Washington Observatory has been sent to me; but as it is quite anonymous, and I have therefore no opportunity of judging of the writer's *bona-fides*, I cannot reproduce it either in letter or substance.

As these notes form rather a miscellaneous collection this month, perhaps I may venture to wind up with a rather interesting recipe, which throws no light on anything astronomical, but tells us something of Oxford a couple of centuries ago:—

Recipe for New College Puddings.

(Circa 1700. From the Record Office.)

> For one duzon take a penny halfe penny white bread and grate it and put to that halfe a pound of beefe suett minced small, halfe a pound of curantes one nutmeg and salt and as much creame and eggs as will make it all most as stiffe as past, then make ym in the fashion of an egg, then lay them into the dish that you bake them in one by one with a quarter of a pound of butter melted in the bottom, then set them over a cleare char cole fire and cover them, when they are browne turne them till they are browne all over, then dish them into a cleane dish, for yr sause take sack suger rose water and butter, pour this over y^r puddings and scrape over fine suger and serve them to the table,

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 March 9.

Mr. E. B. Knobel, *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the January Meeting were read and confirmed.

Mr. Dyson. 115 presents have been received since the last Ordinary Meeting. Among them is a bronze copy of the Sir George Stokes Jubilee Medal, presented by the Registrary of Cambridge University; another volume of the series of the collected Works of Galileo, which is being published by the Italian Government; the first number of the 'Proceedings of the Astronomical and Astro-Physical Society of America'; 'Albategnius, Opus Astronomicum,' presented by the Milan Observatory; a book, presented by Dr. Kinns, called 'Six-hundred Years,' which relates to the church which is in his charge, but is of interest to this Society because of the reference which it contains to Sir Isaac Newton. In the book there is a beautiful copy of the portrait of Sir Isaac Newton, from a picture hanging in the church. There is a paper by Dr. Franz Kugler on the Babylonish Observations of the Moon. Mr. Lynn has presented us with copies of the new editions of his little books 'Remarkable Comets' and 'Remarkable Eclipses.' We have received also a valuable paper by Mr. Whittaker, which is a Report on recent progress in the solution of the problem of the three bodies; another section of Dr. Weinek's 'Photographic Atlas of the Moon'; and, lastly, a volume presented to the Society by Sir William and Lady Huggins, which gives in a sumptuous form the result of their work on Stellar Spectra.

Mr. Newall. As one who has worked on the spectroscopic side of astronomy, I should like to be allowed to say a word or two as to the striking volume that Sir William and Lady Huggins have presented to the Society. It is called an 'Atlas of Stellar Spectra,' a very leading feature in the book being plates of spectra, with which it is replete. These spectra have been produced in the most beautiful manner, and the whole volume will be welcomed by all astronomers engaged in such work.

The President. I would like to call particular attention to one present, which has peculiar interest in certain ways. It is a volume of a series that is now being published by the Milan Observatory, by Prof. Nallino, which contains the great work of Al-Battani—Albategnius, as he is called. It is the Book of the Sabeian Tables, and is in Arabic. The two succeeding volumes will be the Latin translation. This is a most important volume to us, because up to the present time we have not had a copy of the complete work at our disposal. In the 16th century a portion of the work was published, but not in so complete a form as that which is in process of publication. The original MS. is in the Library of the Escurial. The first great work on astronomy was the 'Almagest' of Ptolemy in the 2nd century, and the second was by Al-Battani in the 9th century, the date of this being 879. This work not only contains various chapters dealing with the determination of the position of the Equinox and with the obliquity of the ecliptic, and other matters, but it contains astronomical tables of longitude and latitude of stars; but the reason I mention this publication now is to call attention to the generous action of the Milan Observatory in contributing this important work to astronomical literature.

Mr. Newall then read a paper by *Mr. J. E. Keeler* on photographic observations of Hind's variable nebula in Taurus made with the Crossley reflector of the Lick Observatory. One photograph was taken on 1899 December 6, with an exposure of 3 hours 53 minutes, and another photograph taken with an exposure of 4 hours on December 27 proved to be in exact agreement with the first. Both photographs were good, and the star-discs were almost round. From both negatives positives were made on glass with an enlargement of five diameters, and from one of these the diagram shown was copied.

The President. It certainly seems rather a curious thing that this nebula, which was originally discovered with a 7-inch object-glass in the early fifties, and then again about 1860, should have been invisible in the telescopes which are very much more powerful than that, and should now have been photographed; but there does seem to be a little discrepancy in the matter; so far as I can understand the paper, the position of the nebula does not accord with the position recorded by Hind.

Mr. Newall. I understand this corresponds with the position of Hind's nebula. Struve only guessed the position of the nebula. (Mr. Newall here made a drawing on the blackboard.) The drawing I have made is from Prof. Barnard's paper which he contributed to the Society in 1889, and last year he contributed a paper in which he showed the nebulosity round T Tauri.

Mr. Dyson. I think, if I remember rightly, Prof. Barnard said the nebula was variable.

Mr. Newall. Yes.

The President. There never has been any doubt for many years about the variability of this nebula, but now it has been set at rest by photography, the agency of which was not brought to bear years ago.

Mr. Dyson then read a paper by *Mr. C. T. Whitmell* on the "Maximum Duration possible for a Total Solar Eclipse." This paper, Mr. Dyson said, was of special interest now in view of the length of the eclipse which would occur in May 1901, which would be something over six minutes for observers in Sumatra. Mr. Whitmell gave in his paper a list of some of the longest durations computed from Oppolzer's data and tabulated by Mr. Crommelin. He also gave a table of the latitudes and longitudes of the centre of the region on the Earth where this maximum duration would take place. Mr. Whitmell then considered the question of what conditions were necessary to obtain a long duration, and began by considering what the effect would be of supposing the Moon nearer the Earth. This he concludes will lengthen the duration, and, again, the nearer position of the Sun will shorten the duration of totality. Mr. Whitmell pointed out that for maximum totality five conditions must be fulfilled. These conditions could not all be simultaneously fulfilled, but, as this was the case, a compromise had to be made consistent with the actual facts of nature. Mr. Whitmell stated in his paper that Mr. Crommelin had supplied much of the data, and probably the President would call upon him to say something more on the subject.

Mr. Crommelin. I do not think that there is much to be added to Mr. Whitmell's paper. The eclipses that I worked out for him were deduced from the tables given in Oppolzer's *Canon*. Now from the discussion of a number of eclipses I find that Oppolzer's durations are systematically too long by about 11 seconds. Evidently he uses too large a semi-diameter for the Moon, or too small a one for the Sun, or both combined. The eclipse series of 1901, 1919, &c. is a very remarkable one, and culminates in 1955 with a duration, according to Oppolzer, of $7^{m}\ 26^{s}$, corresponding to a real duration of $7^{m}\ 15^{s}$. The maximum totality in this eclipse occurs near Manila, so this region will have the honour of enjoying the longest totality of any point on the Earth's surface for upwards of 1000 years.

There is a remarkable eclipse cycle of 1805 years, recently detected by M. Oppert, which reproduces very accurately the latitude of the shadow track and the semi-diameter of the Sun and Moon.

Out of curiosity I worked out the duration of totality in the eclipse of 150 A.D., which is 1805 years before 1955. It comes out three seconds longer than the latter, and is the longest eclipse that I have been able to find in Oppolzer's *Canon*.

The President. Have you arrived at an explânation of the source of error in Oppolzer's computation? It is rather singular there should be that amount.

Mr. Crommelin. I have not been able to find an explicit statement as to what semi-diameters he uses in his tables. He does not give the semi-diameters themselves, but certain functions of them. Of course one could deduce what value he used, but I have not done so, but he evidently used an erroneous value. The *Nautical Almanac* now uses Struve's occultation semi-diameter of the Moon, viz. 15′ 32″·65. This, while agreeing with observation for occultations, seems to be too large for total solar eclipses. It gives a duration of totality three or four seconds longer than the American ephemeris, and the American ephemeris gave exactly the right value in India in 1898, so that it would seem better to use their value for eclipses instead of Struve's.

Mr. Dyson. Is the value of the American ephemeris smaller than the value of the mean diameter which would be obtained from observations of occultations?

Mr. Crommelin. I have not in my head what the American value is, but I think it is smaller than Struve's [*].

The President. This reminds me of a paper written by Sir George Airy many years ago on the occultation of stars by the Moon, in which he referred to the telescopic image of the Moon being greater than the actual.

Mr. Newall read a note by *M. Antoniadi*, which gave a description of Saturn as seen by him on a particularly good night, 1899 July 30.

Mr. Shackleton. At a former meeting both Mr. Newall and Mr. Fowler pointed out that the image of the corona seen in "1474 K" light corresponded to an *inner corona* whose outline did not agree with the corona proper. Instead of using a spectroscopic method to photograph the corona in the light of the green corona line during an eclipse, I suggest a way to determine the distribution of *coronium* throughout the corona by employing simple apparatus, and from certain considerations I think this may be done by the use of a camera provided with a suitably coloured screen.

Spectroscopic photographs of recent eclipses show that the coronal light consists principally of a continuous spectrum, the line spectrum, with the exception of "1474 K," being feeble, but which is so intense that I have seen it recorded on ordinary plates supposed to be insensitive to the green. The photograph which I exhibit on the screen is one taken near mid-totality during the 1896 eclipse, and shows that the continuous spectrum has been photographed to a distance of 15′ from the Moon's limb, whilst the green line only reached a height of 5′. Taking advantage of these facts, I have tried to limit the spectrum by the use of a coloured screen to near λ 5303; a simple green, however, is difficult to procure, but by staining two plates with the aniline dyes, methylene blue and tartrazine respectively, I had, on combining them, a colour which allowed a green band of light near the required wave-length to filter through, the orange, yellow, blue, and violet being suppressed.

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[*] Reference to the American ephemeris shows that the value used for eclipses and occultations is 15′ 31″·76, but in the ephemeris for 1902 the value used for this purpose is 15′ 32″·55 on the authority of Peters.—A. C., *March* 16.

From the next slide, which is a solar spectrum taken with and without the suggested green screen, it is evident that only about 5 to 10 per cent. of the light giving continuous spectrum could pass through, whilst the light of the green corona line would be unopposed; since the image of the corona in the light of the former is of much larger area it will be further relatively weakened, so that probably it will be the bright image in "1474 K" light alone that will be recorded with moderate exposures.

No doubt some of the screens used in three-colour photography would be found suitable, but so far I have not yet tried them. It would, however, be very interesting to compare photographs of the corona taken with and without a screen.

Mr. Newall. I should like to ask Mr. Shackleton if he is able to say what the relative exposures given in the last photograph are. In using the diffraction grating, coloured screens are used to get rid of the disturbing effects of superposed spectra. With reference to the use of a yellow screen in photographing the yellow part of the spectrum, the great difficulty of the matter is that the yellow screen in getting rid of the troublesome ultra-violet also weakens the yellow light, so that from three to six times the exposure is required. What additional exposure does Mr. Shackleton think necessary with the green screen?

Mr. Shackleton. When the green screen was interposed I gave the solar spectrum three times the exposure that was given when direct daylight was being photographed. The coloured screen exhibited is the actual screen I used.

Mr. Newall. It would be interesting to know what exposure was given for the corona in 1896.

Mr. Shackleton. 40 seconds. The state of the sky was cloudy at the time.

Mr. Newall. So it seems just possible, as an exposure of $1^m\ 20^s$ is just about what can be given, in May next?

The President. In that green screen you used was there not a considerable amount of blue light passed?

Mr. Shackleton. Very little blue light passed.

The President. There would be some?

Mr. Shackleton. It depends a good deal upon the depth. It let a little of the light through as far as wave-length 5000.

The President. I suppose a liquid screen would be impossible?

Mr. Shackleton. I am afraid that would give too much distortion in photographing the corona.

The President. With copper acetate you would get very good results, much better, I think, than with aniline dyes.

Dr. Spitta. Have you not found that aniline dyes fade very much in a strong light? They fade very quickly. What might be suitable for one occasion with exposure to ordinary daylight might not be the true thing for the next. I have not used them for my particular purpose because they fade and lose their intensity.

Mr. Shackleton. These dyes are used in dyeing wool, and are supposed to be almost permanent.

Mr. Newall. May I take the opportunity of calling attention to one point on which there has been a misapprehension, and in which a means of research may be overlooked in consequence of the failure of my attempt to get a photograph of the corona spectrum in India in 1898? It has been said several times that my failure was attributable to the relative faintness of the blue line. I do not think the evidence proves this. (Blackboard.) None of the photographs of the spectrum of the corona that were got showed extension in any part of the spectrum greater than five minutes from the limb of the Moon, and my observations dealt with light coming from the corona at eight minutes from the limb, and only with those parts, so that in making comparison of my attempt with the successful observations of the green and blue, it ought to be taken into account that the things compared are different, and the evidence available does not, I think, show that the blue line is weaker than the green line. It would be a pity if these results were misunderstood and stood in the way of observers making use of wave-length 4233 in getting information about the corona. My own belief is mainly based on the study of Capt. Hills's beautiful photograph, which I think shows that the blue line is as strong as, if not stronger than, the green line of the corona. I hope that Mr. Shackleton will have an opportunity of testing the serviceability of the screen in the eclipse that is coming.

Mr. Shackleton. Referring to Capt. Hills's photograph, I think the green line comes out without any continuous spectrum.

Mr. Newall. I am speaking now of the original negative, from which one can get a much better idea of the relative intensity, and I feel convinced there is little difference between the intensity of the green and the blue lines.

Mr. Evershed. I think the green line must be relatively brighter.

Mr. A. Fowler. I can confirm what Mr. Evershed says. In the prismatic cameras we have used we have the green line invariably stronger than the blue line, and good results are obtained with exposures of about 10 seconds. I think the indications of the green line considerably stronger than the blue.]

Mr. Newall. I should like to ask Mr. Fowler whether the green line was in the middle of the plate in the case to which he refers. I do not know whether the green has been favoured at the expense of the blue. It is an interesting point, and I am glad to have referred to it, if only for the sake of having elicited the very strong opinion of Mr. Fowler and Mr. Evershed.

Mr. Fowler. The green ring was about as much from the edge of the plate as the blue ring. The total visible effect seems to depend upon the extension of the image as well as the brightness.

Mr. Newall. You do not get the photographic effect of the superposition of the bright lines on the different parts of the continuous spectrum on the same basis. I am not prepared to say which way that would tell in the matter, but it seems to me quite possible that the green line *appears* more strong in contrast because of the absence of the continuous spectrum.

Mr. Fowler. Surely that means that the green line is very much stronger as compared with the continuous spectrum. If the continuous spectrum does not come out and the green line does, that means that the green line is brighter.

Mr. W. W. Bryant then read a note on a possible occultation of A Geminorum by Venus on 1900 May 27–28 next, and said he would send the information to the Australian Observatories in time to make observations. It would happen near sunset in New Zealand, and might be used as a means of determining the solar parallax.

Mr. Dyson then read a paper by *Prof. Turner* on a simple method of comparing the places of stars in the Bonn 'Durchmusterung' with those on astrographic plates. The paper, he said, was essentially a matter of computation, and was a means of simplifying the work of comparison. The accuracy of the 'Durchmusterung' was such that no very accurate comparison was necessary, but it seemed desirable now that astrographic work was in progress that every possible comparison should be made with all previous observations. With regard to stars down to the 9th mag., comparison of places with those of the 'Astronomische Gesellschaft' Catalogues were made in the course of formation of plate constants. The comparison with Argelander's catalogue extended this to fainter stars. Prof. Turner's paper simply dealt with the easiest method of making the comparison with the least amount of labour.

Mr. Newall then read a short note on the binary system of Capella, and said that almost immediately after his brief note had been presented to the Society in November, an announcement reached this country from Prof. Campbell that a similar discovery had been already made by him at the Lick Observatory. The present note was again of a preliminary nature, and its object was to lay before the Society some results of the observations made at Cambridge. Shortly after the new four-prism spectroscope was mounted at Cambridge in July last, it was noticed that the spectrum of Capella was ill-defined, and that it varied in a curious manner from night to night, and it soon became clear that the peculiarities were real and not due to instrumental defects, for photographs were obtained of the spectra of other stars, such as Procyon and Sirius, which came out very well indeed. After some remarks on the changes in the velocity of one of the components, illustrated by a lantern-slide, which showed how the observed velocities indicated an approximately circular orbit described in 104 days, Mr. Newall stated that he deferred publication of results from which the relative masses of the components might be deduced until observations at a particular phase in the period had been secured, for those in December had been missed in consequence of bad weather. It was interesting, he stated, to find that the best-defined photographs of the spectra obtained by the Potsdam observers in 1888 were secured at dates which were separated by an exact multiple of half-periods (52 days) from the epochs at which the spectrum appeared single in 1899. He had the impression that Mr. Burnham had carefully searched for evidence of duplicity in Capella and had not found it. The components were, therefore, probably less than a tenth of a second apart. It might accordingly be assumed that the orbit was at least 116 million miles in diameter, if Dr. Elkin's parallax were adopted. The orbit was not "edge-on" nor "face-on" to the observer, but it was probably at an angle of at least 26 degrees. One of the objects in bringing this note before the Society was to call attention, before Capella was too much involved in twilight or daylight, to the fact that it was quite possible that Capella might be found to be a telescopic binary. The spectroscopic observations showed that the masses of the two components were about the same, and also that the brightness of the components was about the same. If darkened glasses were used it might be found that Capella could be seen as a close double star.

The President. I am sure we congratulate Mr. Newall upon this very remarkably interesting research and discovery that he has made. We know how assiduously he has been devoting himself to this work of determination of the velocity of stars in line of sight, and this reward for so much labour is one we all congratulate him upon.

Mr. W. W. Bryant. Can Mr. Newall give us an idea of about what the separation would be?

Mr. Newall. One cannot tell that until one gets the inclination of the orbit, but the limit I dealt with in the note, viz. a tenth of a second of arc, would involve a diameter of 116 million miles for the orbit, whilst the least possible diameter is 52 million miles.

Mr. Bryant. Mr. Burnham has spent several nights at this star and I do not think that at Greenwich we have a better chance of separating it than he has.

Mr. Newall. The new point as it strikes me is that the components are almost equal in brightness, and, so far as I can make out, Mr. Burnham has mainly looked for minute components, and that has prevented cutting down the light by a dark glass. In using a dark glass we get the whole resolving power of the big telescope available for looking for two nearly equally bright stars.

Mr. Bryant. When may the greatest elongation be expected?

Mr. Newall. I am not prepared to say that off-hand *, but it seems to me, from the consideration I have made of the matter, that one was more or less independent of the moment of the phase in the period. In the absence of knowledge of the inclination and of the position of the line of nodes, I do not think one can state that exactly.

Rev. Dr. Kinns. May I say a word in reference to my book, which has been already mentioned this evening? There is in it full particulars of a valuable MS. I found at the British Museum, which contains an account of Sir Isaac Newton's work at the Mint, written by Mr. Hopson Haines in 1700. There is a portrait of Newton in my church, where, I am told, it has been for 100 years, and I have included it in my book, which consists of historical sketches of eminent men and women who have come in contact with the church for 600 years.

The President. We shall read that chapter with great interest as a page in the history of astronomy. The Meeting is now adjourned until Wednesday, April 11.

The following papers were announced and partly read:—

J. E. Keeler. "Photographic Observations of Hind's Variable Nebula in Taurus made with the Crossley Reflector of the Lick Observatory."

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon for the Second Half of 1900."

H. H. Turner. "On a Simple Method of comparing the Bonn 'Durchmusterung' with Photographic Plates."

C. T. Whitmell. "The Maximum Duration possible for a Total Solar Eclipse."

W. W. Bryant. "Note on a Possible Occultation of A Geminorum by Venus, 1900 May 27–28."

H. F. Newall. "On the Binary System of Capella."

W. Shackleton. "The Use of a Coloured Screen in Photographing the Corona."

F. C. Penrose. "Graphical Method for the Determination of the Local Times of Contact in a Solar Eclipse."

E. M. Antoniadi. "Observations of Saturn made at Juvisy Observatory in 1899."

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* *Note, March* 21.—It appears that the best chance of detecting elongation will first occur about April 13, or say between April 3 and April 23.—H. F. N.

The following Candidates were proposed for election as Fellows of the Society :—

Alex. Foote, F.S.A.Scot., 111 Warwick Road, Earl's Court (proposed by A. Fowler).

Désiré Ernest Lebon, Professeur de Mathématiques au Lycée Charlemagne, 4 bis Rue des Écoles, Paris (proposed by C. Flammarion).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The fourth ordinary Meeting of the Session of the British Astronomical Association was held on February 28th, at Sion College, *Mr. W. H. Maw*, President, in the Chair.

The names of five Candidates were read and passed for suspension and the election by the Council of eleven new members was confirmed.

Mr. Maunder read a paper by *Mr. A. Stanley Williams* on "The Double Canals of Mars," dealing with some notes by M. Antoniadi on a previous paper of the writer's. Mr. Williams thought he had given sufficient reasons to show why a particular canal might be described as double by one observer, and only single by another at the same time. Differences of eyes, telescopes, magnifying-powers, weather, and skill and training of observers appeared amply sufficient in themselves to explain disagreements of this kind, without it being necessary to seek for other causes. The writer disputed also the statements by Capt. Noble and Mr. Holmes to the effect that the most wonderful things on Mars had been seen when the planet was a long way off from the Earth and badly situated.

Mr. Maunder said it was very interesting to have this question of the double canals of Mars discussed by two such observers as M. Antoniadi and Mr. Stanley Williams, who were certainly two of the keenest observers of planetary detail now alive. After a careful examination of a large number of drawings made during the opposition of 1892, he (Mr. Maunder) could not resist the conclusion that, wonderful and almost incredible as some of Mr. Williams's drawings appeared, he did really observe and see the things which he drew and described; as to M. Antoniadi, no one could study his drawings without seeing how faithful an observer and splendid an astronomical artist he was. This made the discrepancy between the two the more remarkable, and the clearing up of the discordance would undoubtedly be of the utmost value.

Mr. Wesley said that M. Schiaparelli discovered the canals of Mars at the opposition of 1877, but he did not see them until some time after opposition was past, and he kept seeing them better for some months after that. It appeared that as the apparent diameter of Mars grew smaller Schiaparelli saw more canals.

Mr. Nelson said that with a microscope the doubling of structure was quite a common phenomenon. He was of opinion that spherical aberration in large telescopes was much greater than was generally supposed. He was glad to be able to report that flint and crown glasses, which had their dispersion powers proportional throughout the spectrum, had lately been constructed in Germany, and that, therefore, apochromatism, which had been enjoyed in the microscope for the last 15 years, was now possible in a telescope objective.

Mr. Seabroke said that the increase in the diameter of the telescope did not necessarily increase the spherical aberration. The spherical aberration of the lens varied as the square of the diameter over the focal length.

Mr. Maunder, referring to Mr. Wesley's remarks, said he believed that Schiaparelli observed the canal Titan at a time when the diameter of Mars was very little over 5 secs. of arc. With regard to his own observations, he had never been fortunate enough to see a canal doubled, although when his eyes were tired or strained he had often seen the lines in the spectrum doubled. He thought Mr. Stanley Williams was too skilful and experienced an observer to be misled by spurious duplication.

The President hoped that Mr. Stanley Williams's paper and the discussion on it would lead to their receiving another paper from M. Antoniadi. They had by no means got to the bottom of the subject yet.

Mr. Maunder handed round a couple of opera-glasses which had been sent for exhibition by their ingenious fellow member Mr. Thorp, of Manchester. One of these was fitted with a prismatic grating before one of the object-glasses, the other with one at one of the eyepieces for the observation of the coronal spectrum during the coming eclipse.

Mr. Petrie read a paper by the *Rev. D. R. Fotheringham* on "The Rise and Fall of Astrology." The writer maintained that for more than 5000 years not only were astronomy and astrology closely related, but that as late as 1560 they were absolutely identical, and that even now it was very difficult to draw a hard-and-fast line between them. It was desirable that some persons at least should study astrology, as without a knowledge of it some of our oldest literature was unintelligible.

Mr. Crommelin thought it was rather an abuse of the established meaning of terms to talk about things like the tides or the sympathy of the terrestrial magnetic cycle with sun-spots as astrology. The term "astrology" was generally used in the sense of the supposed effect of the heavenly bodies upon purely human affairs.

Mr. Maunder said we owed one debt to the astrologers—that they had kept up the science of observational astronomy for some 2000 years. Theoretical astronomy had made absolutely no progress during that period, but such tables of the motions of the planets as had been handed down to us we owed to the astrologers.

Mr. J. D. Hardy read a paper on "The Enlargement of Heavenly Bodies." He said that as a consequence of refraction alone we did not see the Moon—or any other celestial body—where it really was, so by the reflection of the rays the edges, and necessarily the whole body, were extended by the angle made by the rays before reaching the eye. We did not see the Moon where it really was, but where the angles of reflection made it appear to be, and the combined effects of those refracted and reflected rays was to increase the apparent size of the Moon.

Mr. Maunder read a note by the *Rev. Joseph Allen* maintaining that the apparent enlargement of the Sun and Moon when near the horizon was purely illusory, and suggesting the following as a possible explanation of the illusion: when we looked at the Moon high up in the sky we had not the same sense of distance as when we saw it near the horizon—it looked further off in the latter position; and because it subtended the same angle to us in both positions, we unconsciously regarded it as larger at the horizon, and therefore it seemed larger.

Mr. Seabroke considered the phenomena purely physiological. The size on the retina of the image of the Moon, or the distance between any two stars when seen on the horizon and when seen in any other position, must be exactly the same if they eliminated atmospheric refraction; but it was a matter of custom, physiologically, he thought, to imagine objects to be larger when seen near the horizon than when seen in any other position, just as it was the custom to combine into one the two images seen by each eye.

Mr. Maunder remarked on the difficulty of estimating the size of distant objects. Most people, he said, would suppose that the Moon would obliterate the whole of the Pleiades, but as a matter of fact it would require two or three Moons side by side to do that.

The discussion was continued by *Mr. Maunder*, *Mr. Crommelin*, and *Mr. Wesley*.

Mr. Hardy replied, and some enlarged photographs of portions of the Moon taken at the Lick Observatory were thrown upon the screen and explained by *Mr. Goodacre*.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, March 21st, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. C. Theodore Williams*, President, in the Chair.

Reference was made to the loss which the Society had sustained by the death of Mr. G. J. Symons, F.R.S., who had held the office of Secretary from 1873 to 1899, except for the two years 1880–81, when he was President. At the Annual Meeting on January 17 last he was elected President for the second time in order to preside over the Jubilee Celebrations of the Society next month. Owing to being seized with paralysis on February 14, he had to resign the Presidency. He never rallied, but he died on the 10th instant. A vote of condolence with his relatives was passed by the Meeting.

Twenty-seven new Fellows were elected, as well as two Honorary Members, viz., Mons. Albert Lancaster, Director of the Belgian Meteorological Service, Brussels, and Gen. M. A. Rykatcheff, Director of the Central Physical Observatory, St. Petersburg.

The following papers were read:—

"The Ether Sunshine Recorder," by *Mr. W. H. Dines, B.A.*

"Remarks on the Weather Conditions of the Steamship Track between Fiji and Hawaii," by *Capt. M. W. C. Hepworth.*

"Comparison by Means of Dots," by *Mr. A. B. MacDowall, M.A.*

A Comparison of Tycho Brahe's Meridian Observations of the Sun with Leverrier's Solar Tables.

[Continued from p. 135.]

THE SUN OBSERVATIONS.

THE 'Historia' contains nothing as regards these observations more than a bare statement of the altitude and the name of the instrument. A correction for zero might have been applied mentally (as is the custom now-a-days among sailors), which would inevitably result occasionally in its being applied with the wrong sign.

However that may be, it is mere conjecture how the errors of z. d. of four or five instruments could all change by comparatively large amounts in the same direction in an interval of a day or two. This frequently occurs, including the great mural quadrant. It is quite certain that the altitudes have not been corrected for refraction or parallax.

Although the altitude is sometimes given as measured with seven or eight instruments (the average number is three), no mention is made how many observers there were. It would have been quite possible for one observer to point several while the object was within a few minutes of the meridian, and read the arcs afterwards.

There are a great number of misprints in the volume, but many of them are easily corrected, such as dates, whole degrees, tens of minutes. Misprints in the units of minutes could not be corrected with certainty.

There were 19 instances out of 2500 of the error of z. d.

exceeding three minutes; these I rejected, although they were all of the same sign.

The Sun's meridian altitude was observed on 890 days during the nine years under discussion. After 1590 there is a great falling off in this class of observation, so much so that nothing would have been gained by including the fragmentary years.

The following is the method I adopted for finding the tabular declinations from Leverrier's Tables:—

The Sun's apparent longitude was taken out directly for every isolated day of observation, and for every two days when the observations were more or less continuous. The lunar equations of the longitude were included for the first two years, afterwards omitted as of no importance to the mean of the year. The perturbations produced by the four principal planets were taken out for intervals of twenty days and interpolated with second differences, very small terms being omitted. As the longitudes were correct to about 1″, the declinations have an accuracy exceeding that required. This work was not done in duplicate. The differences guarded against mistakes in the continuous runs, while the isolated days were compared with an accurate ephemeris for 1590 prepared for every day. The declinations had to be reduced to apparent noon at Uraniburg, which necessitated the calculation of the approximate equation of time. The longitude of Uraniburg was assumed to $0^h\ 41^m\ 19^s$ east of Paris.

The observed altitudes being corrected for Bessel's mean refractions and for parallax, the Sun's declination was inferred for each observation by subtracting the co-latitude 34° 5′ 32″. Thus was obtained the apparent correction required to the tabular declination ($\Delta\delta$).

Each observation furnishes an equation of the form

$$\Delta\delta = a\Delta\odot + b\Delta\omega + \Delta z,$$

where $\Delta\odot$, $\Delta\omega$, and Δz are the corrections required to the tabular mean longitude of the Sun, mean obliquity, and mean zenith distance respectively, and

$$a = \cos\odot\,\sin\omega\,\sec\delta,$$

$$b = \sin\odot\,\cos\omega\,\sec\delta.$$

The values of the coefficients a and b were tabulated to three decimal places for each degree of the Sun's longitude. Two decimal places are sufficient for a daily equation, becoming three in a monthly mean.

If n be the number of instruments used on any day, the weight given to the equation for that day is $\sqrt{n}$.

With these daily equations so weighted a monthly equation was formed, or twelve equations of conditions each year for determining the values of $\Delta\odot$, $\Delta\omega$, and Δz.

The number of observations in the different months was extremely unequal. Some months have only one or two observations, others fifty or more. The weight given to each monthly equation was the square root of the sum of the daily weights.

These twelve equations, thus weighted, were solved by the method of least squares, with the following results:—

	$\Delta\odot$.	$\Delta\omega$.	Δz.	Number of days of obs.
1582	$-6''$	$+7''$	$-42''$	95
1583	-9	$+29$	-47	128
1584	$+9$	$+20$	-42	110
1585	$+4$	$+28$	-52	83
1586	$+17$	$+36$	-35	98
1587	-11	$+30$	-19	89
1588	-2	$+7$	-33	83
1589	-10	-18	-13	72
1590	-30	-16	-23	110
Means...	-4	$+14$		

The value of $\Delta\odot$ is somewhat less than its probable error as inferred from the discordances of the different years, so that Leverrier's mean longitude of the Sun perfectly satisfies Tycho's observations.

The large value of $\Delta\omega$, implying as it does that the annual diminution of the obliquity is $0''.05$ greater than its known most accurate value, at once suggests a systematic error between the observations at the summer and winter solstices. To throw some light upon this point the value of $\Delta\delta$ for each month of the nine years is shown in the subjoined table (p. 168).

The numbers in the right-hand column being projected on a chart of squares and a free curve drawn, it is seen at once that for the summer solstice $\Delta\delta$ is about $-20''$, and for the winter $-50''$, a simple curve satisfying the figures very approximately.

It is stated above that the observed altitudes were corrected for *mean* refraction. At the winter solstice the mean temperature of Copenhagen is $-0^\circ.2$ Reaumur ($31\frac{1}{2}^\circ$ F.). Taking the mean daily range as 7° R., the mean temp. at noon would be about 3° R. (40° F.), which for altitude 11° gives the correction of mean refraction about $+15''$. The value of $\Delta\delta$ above for the winter solstice would thus be *increased* to $65''$, making the discrepancy between summer and winter $45''$, if the propriety of this correction be admitted *.

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* It would appear from Tycho Brahe's 'Meteorological Daybook' that only one of these winters was exceptionally cold, and one was singularly free from frost.

Values of Δδ for each Month.

	1582.	1583.	1584.	1585.	1586.	1587.	1588.	1589.	1590.	Means.
January	−32″	− 49″	−72″	− 45″	−58″	−42″	−42″	−28″	−16″	−43″
February	−42	− 55	−49	− 52	−38	−38	−39	−20	−16	−39
March	−60	− 51	−24	− 54	−35	−15	−39	−17	−54	−39
April	−35	− 30	−27	− 43	−13	− 4	−22	0	−46	−24
May	−56	− 32	−31	− 38	−53	− 2	−32	..*	−38	−31
June	−31	− 7	−21	− 22	+ 5	+ 7	−15	+10	−23	−11
July	−53	− 53	−25	− 26	− 4	+11	−12	0	−17	−20
August	−29	− 34	−34	− 44	−23	+ 7	−33	− 2	−28	−23
September	−39	− 31	−47	− 54	−36	−27	−38	−17	−37	−36
October	−40	− 63	−58	− 75	−69	−32	−55	−19	−35	−50
November	−62	−101	−62	− 79	−71	−39	−20	−20	..*	−50
December	−49	− 83	−53	−123	−63	−50	− 8	+ 5	+21	−45
Means	−44	− 46	−42	− 55	−38	−19	−30	− 9	−26	

* No observations in 1589 May, and only one in 1590 November.

It would be interesting to know to what extent the various instruments participated in the annual inequality above mentioned, but the employment of the majority of them in a very irregular manner, as regards continuity, makes it hardly possible to get independent means of any value for any instrument except the great mural quadrant. Being away from home I have only the information necessary for the last four years, 1587–1590.

Monthly Values of $\Delta\delta$ for the Mural Quadrant.

	1587.	1588.	1589.	1590.	Means.
	″	″	″	″	″
January	−45	−62	−27	−40	−43
February	−36	−42	−27	−38	−36
March	−17	−35	−14	−42	−27
April	− 3	−14	+ 9	−26	− 9
May	0	−12	...	−25	−12
June	0	0	+16	+ 3	+ 5
July	+ 7	+ 1	+ 7	+21	+ 9
August	−10	−24	− 1	+ 2	− 8
September	−42	−24	−40	−28	−33
October	−47	−60	−51	−34	−48
November	−54	(−65)	−59	...	−59
December	(−42)	−33	−82	−62	−55

The means in the last column may be said to be identical with the means obtained by weighting by number of observations—three of them differ 2″ only. When these means are projected on a chart of ruled squares, a free simple curve is found to pass exactly through nine of the points—April, May, and July are within 10″ of it. Thus for this instrument there is a maximum value of +6″ about June 30, and a minimum of −60″ about November 7, an annual inequality of 66″, which would be still larger if the refractions could be corrected for temperature.

For the *Quad. Max. Chalyb.* there are no useful observations in 1587, none in 1588 October, or 1589 May; and for the *Quad. Volub.* none in 1589. The monthly means for each of these instruments depend therefore upon three years only, as follows, the three years being considered a single period:—

	Q. V.	Q. M. C.
January	−18 18	− 9 18
February	−43 29	−23 27
March	−44 30	−42 29
April	−36 25	−41 26
May	−24 27	−46 19
June	−32 37	−12 30
July	−22 18	−12 17
August	− 7 27	−30 16

	Q. V.		Q. M. C.	
September	−33	22	−31	24
October	−16	17	0	13
November	−10	6	+11	10
December	+21	12	+22	13

The number of observations is indicated by the small figures.

For the *Quad. Volub.* there are no certain indications of an annual inequality, but for the *Quad. Max. Chalyb.* there appears to be *minimum* of −40″ about the middle of April, a *maximum* of +10″ at the end of November, with an intermediate secondary *max.* and *min.* It is to be said in favour of both these results that it makes very little difference which way the means are found, by observations or by months.

It might be thought that the almost universal appearance of the negative sign in the value of $\Delta\delta$ (or, what is the same thing, in the correction required to the observed zenith distance) is due to the Sun having been observed very generally off the meridian.

Suspecting this I have examined the zero of azimuth as obtained in 1582 by stars. These are 41 in number, almost all being records of the star's altitude when on the supposed prime vertical. The places of the stars were taken from Auwers' Bradley, as before described in the Latitude section. Four of these give such wild results it is probable the figures are misprinted, or the stars not certainly identified. The assumed position of the prime vertical on the azimuth circle has an apparent error of 7′ three times, of 4′ or 5′ four times; the remaining 30 less than 3′. It is therefore pretty certain that there was no large error in the assumed position of the meridian, and it is probable it never much exceeded 1′.

Tycho Brahe was perfectly aware that his altitudes of the Sun were affected by some constant error, which is probably the reason the number of instruments used was increased in 1586 to six or eight, without, however, reducing the mean error of altitude, while the errors of longitude and obliquity were disastrously affected. Nevertheless, the pretty constant use of the *Quad. Max. Chalyb.* has modified in the long run the annual inequality, which would appear to be mainly caused by the temperature affecting the arc of the mural quadrant; but the *maximum* and *minimum* occur some two months earlier than one would expect if they were caused by the temperature of the air.

OBSERVATIONS OF HEVELIUS IN 1679 OF THE SUN'S MERIDIAN ALTITUDE.

These observations, 94 in number, printed in the *Annus Climactericus*, extend from 1679 January 9 to September 26—the very day of the great catastrophe that destroyed not only his immense establishment, but the whole of his manuscripts and almost the entire edition[*] of his printed observations—his life's work.

Compared in the same manner as Tycho Brahe's with Leverrier's Solar Tables, assuming the latitude to be 54° 22′ 10″[†], the corrections required to the observed zenith distances are shown in the following table :—

1679.	Obs.	Δz.	1679.	Obs.	Δz.
January 9–15	2	$+42''$	June 3–9	6	$-48''$
February 2–6	4	$+80$	„ 10–20	6	-33
March 11–18	6	$+51$	„ 21–27	6	-11
„ 19–30	6	$+48$	„ 28–July 6	7	-2
„ 31–April 11	6	$+16$	July 8–20	6	-5
April 12–30	6	$+38$	„ 28–August 4	5	-33
May 1–13	6	-10	September 6–18	4	-30
„ 14–24	6	-72	„ 19–26	5	-10
„ 27–June 2	6	-104			

The mean $\Delta z = -8'' \pm 5''$.

It would be a waste of time to infer the place of the ecliptic from such observations. They are affected by an annual inequality much larger than that for any one year of Tycho Brahe's, but of opposite sign. Projected on ruled squares, the curve indicates $-45''$ about July 13, and $+60''$ about January 20. These numbers, however, are very uncertain. G. L. TUPMAN.

A Possible Cause of the Variability of Stars[‡].

IN our study of nature it is sometimes advantageous to speculate as to possible causes of observed phenomena in cases where we are not yet in a position to institute an investigation which would entitle us to have no doubt as to the true cause. The cause suggested by the speculation may possibly prove to be the true cause; and if not, it is at all events likely to bear a valuable analogy, as regards the laws of its operation, with the mode in which the real cause operates.

Prof. George Darwin's theory as to the origin of the Moon is one of the most striking examples of such a speculation; and it was on reading it some years ago that an application of the same principles of action to account for variable stars occurred to the present writer. He now publishes the speculation because it seems to offer a satisfactory explanation of some remarkable facts recently brought to light which have attracted much attention, viz. that as many as forty stars of the Cluster Messier V., nearly one-twentieth of all the stars in the cluster, have been found to be variables possessing periodic times, light-curves, maxima of brightness, and minima of brightness, which, though not the same, do not differ much from one star to another.

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[*] Lalande states ('Bibliographia') that he knew of 30 copies that had been sent out before the fire. Where are they now?

[†] 'Podromus,' 9, 10, with correction for refraction.

[‡] From *Nature*, 1900, March 8.

As an introduction to the explanation which is about to be suggested, it will be convenient to refer to a very early experience of the writer. Potatoes used formerly to be boiled in open pots over a naked fire, and a phenomenon then presented itself, which he often watched when a boy with wonder, until at length, to his satisfaction, he perceived the very simple cause to which it is due. The water at almost equal intervals of time swelled up, and a little boiled over; it then subsided and boiled more tranquilly. These phases were repeated with surprising regularity, making the whole a definite quasi-periodic phenomenon; although upon a closer scrutiny it was found that the intervals, while nearly, were not exactly equal, neither were the recurring phases of the phenomenon accurately alike. Further experience showed that this example of quasi-periodicity is not exceptional, but one of a great body of quasi-periodic phenomena which occur in nature. With one of them we are here concerned, viz. with that which goes through its evolutions on our Sun, and manifests its approximately periodic character in the eleven-year period of sun-spot frequency.

It is obvious that the movements and other events to which sun-spots are due would only need to become a good deal more energetic to render our Sun a perceptibly variable star with a period of eleven years. Now, a cause which may perhaps render them more effective is this. According as the Sun or other star shrinks, its sun-spot or star-spot period will presumably undergo some change; but it is very unlikely that this change will follow the same law as that which governs the progressive shortening of the period of natural pulsation within the entire mass of the star. Accordingly, at a certain epoch in the history of the star, the two periods may approximate to one another. Thereupon the events producing star-spots are likely to acquire augmented intensity, which may render the star a variable star for a long cosmical period; in fact, until further shrinkage shall have slowly destroyed the adjustment.

Nor is it necessary that the two periods—that of star-spot frequency and that of natural pulsation within the star—shall become identical. The fluctuations in the number and size of star-spots will probably become exaggerated whenever the two periods in question become related in other simple numerical ratios. Accordingly, a star in the whole course of its life-history may at more than one stage become a variable star, although the most conspicuous fluctuation of its brightness, and that which is represented by the simplest form of light curve*, will only occur when the periods become equal.

In Messier V.—the great cluster in Virgo—the evidence which is published by Prof. Bailey in the *Astrophysial Journal* of last November establishes the fact that at least forty of its stars, or nearly one-twentieth part of all the stars of the cluster, differ but little from one another in brightness, and exhibit other resemblances which indicate that these stars are now very much alike in their physical condition. It is, therefore, in a considerable degree probable that at a remote epoch in the past they were so nearly in the same physical condition as to have then had pretty nearly the same brightness, the same star-spot period, and the same period of internal dynamical vibration. This amount of resemblance between so large a proportion of the stars of the cluster will not seem improbable to any one with experience of the appearances of star clusters, in many of which a conspicuous feature is the very notable proportion of the stars which are of one or of some few definite magnitudes.

If, then, these forty stars were originally nearly alike, they would continue so during their subsequent history. They would all shrink in the same way, they would continue at each subsequent epoch to have nearly the same star-spot period, and also to have all of them approximately the same period of natural internal pulsation; and would accordingly all arrive nearly simultaneously at that stage when these periods approximate. They would then all of them become variables, and under precisely the circumstances which have been observed, viz. with the simplest form of light curve, and with some approach towards having the same maximum of brightness, the same minimum, and the same period of fluctuation.

It has been observed above that in the successive adjustments that may arise while a star is shrinking, some may be of a kind to lead to variability with more than one minimum in each cycle, while the principal adjustment (where the two periods become the same and not merely simply related) will have only one minimum in each cycle. Instances of both are presented by known variable stars; though naturally the second case is that which has been most noticed because it is, when it occurs, that the brightness of the star exhibits the most conspicuous range of fluctuation.

G. Johnstone Stoney.

George James Symons, F.R.S.

By the death of Mr. G. J. Symons a most distinguished man has been removed from the ranks of science. George James Symons was born in London on 1838, August 6, and began to take interest in meteorological pursuits at a very early age. His first insight into serious meteorological work, however, may be considered to date from his introduction into the Meteorological Department of the Board of Trade, then recently constituted under the direction of the late Admiral Fitzroy. An indication of Mr. Symons's application to his official work may be gathered from an elaborate report on the anemometry of Bermuda, rendered to the Board of Trade in 1861 and published in the 8th number of the Meteorological papers issued by the Board.

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* There is quite enough of correspondence between the light curve of those variable stars which have one minimum in each cycle, and the curve of sun-spot frequency, to create an appreciable presumption in favour of the speculation of the present paper.

At the age of 21 he began the work with which his name will ever be associated, viz. the collection and publication of rainfall statistics under the title of 'British Rainfall.' The first issue of this remarkable labour of love, stated by Mr. Symons to have been "the product of his leisure hours," appeared in 1860, and embraced returns from about 500 stations. The work grew so rapidly that he was soon compelled to devote the whole of his time to it, and to sever his connection with the Meteorological Department. In 1865 the number of reporting stations had increased to 1000, in 1868 to 1500, in 1876 to 2000, in 1880 to 2500, in 1889 to 3000, and in 1898 to nearly 3500. It must not be forgotten that the collection, examination, and tabulation of these records including a large amount of calculation and a vast amount of correspondence, did not by any means constitute the whole of Mr. Symons's work, which involved also the inspection of the observing stations and gauges, the instruction of the observers, and a constant and painstaking search into old books and newspapers for buried records. It is probable that by his assiduous researches into old records he had accumulated the largest and most valuable collection of rainfall observations extant, and in his last-published volume (that for 1898) he was seriously taking into consideration the re-calculation of his data in order to completely represent the rainfall fluctuations in Great Britain during 175 consecutive years—that is to say, back to the year 1725. He thus became recognized as the greatest authority upon rainfall in the country, and was frequently called as a witness on questions relating to water-supply. In the last few years his labours have been shared by H. Sowerby Wallis, who will continue to carry on the work. In 1866 he started a monthly meteorological magazine, which is still running a successful career.

He was elected a member of the British Meteorological Society (now the Royal Meteorological Society) on 1856, March 25, was Secretary of the Society for many years, and President from 1880 to 1882. At the anniversary meeting in 1900 being the senior Fellow (with the exception of Mr. Glaisher and Mr. Lowe), he was elected President for the second time, to commemorate the Jubilee of the Society, but was compelled to relinquish office on account of failing health. On 1878 June 6 he was elected a Fellow of the Royal Society, and he was also a member of several other learned Societies. In 1883 he became Chairman of the Committee of the

Royal Society appointed to investigate the Krakatoa eruption, and drew up the Committee's report on the subject. He was a regular attendant at the Meetings of the British Association, taking an active part in the business of the meteorological sections. In 1897 the Albert Medal of the Society of Arts was awarded to Mr. Symons for his eminent services in establishing and carrying on rainfall investigations at a great number of stations in the British Isles during a period extending over nearly 40 years. He took great interest in the Royal Botanic Society and held office on the governing body. He was a voluminous writer, principally on rainfall topics, but also found time in addition to edit among others: 'The Report on the Krakatoa Eruption of 1883, August,' already mentioned; 'A Meteorological Register kept at Sunbury by the Rev. James Cowe from 1795 to 1839'; 'Theophrastus on Winds and Weather Signs,' &c., &c. An active and intelligent worker, not only in his own particular branch, but in many other departments of science, he attained the rare distinction of passing through life without making a single enemy. He died from a paralytic seizure on 1900, March 10, at the age of 62 years, and was buried at Kensal Green Cemetery. He leaves no family; his only child died in infancy, and his wife pre-deceased him by fourteen years.

W. C. Nash.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Comet of 1677, observed by Hooke.

Gentlemen,—

In No. 3624 of the *Astronomische Nachrichten* (vol. cli. col. 395), Dr. Holetschek remarks that in Wolfers's translation of part of the 'Opuscula' of Newton it is stated that Hooke observed the comet of 1678, and that it was as bright as a star of the first magnitude, and equal to Saturn when that planet was near the horizon. As he could not make the description agree with La Hire's comet of 1678 he suspected an error of date, and at first thought the great comet of 1679–80 might be referred to, but found such was certainly not the case. A marginal note in Pingré (vol. ii. p. 197) refers to a comet observed by Hooke with the date 1678. But my letter in the *Observatory* for November 1899 (vol. xxii. p. 393) led Dr. Holetschek rightly to conclude that 1678 in Pingré meant, not the date of the observations of the comet, but that of the publication of Hooke's 'Cometa.' He then conjectured, as I had referred to Hooke's mention of the comet of 1664, that that was the one alluded to by Newton in the 'Opuscula.' Such, however, is not the case. The greater part of

Hooke's treatise is on the comet of 1677, and that is really the one intended, the 1678 being either a slip of Newton's or an error of the printer; the work, it will be remembered, was not published until after the author's death. Hooke, however, says that the comet was as large as a star of the first magnitude, but fainter, and compares it, as Newton says, with the light of Saturn. He first observed the comet on April 21 (Old Style), having heard of its appearance the day before; the discoverer, as is well known, was Hevelius, on April 27 (New Style).

Another circumstance connected with this matter may be of some interest. The portion of the 'Opuscula' referred to above is called "De Mundi Systemate," the title of the third book of the 'Principia.' It was originally intended to form that book, but on second thoughts Newton re-wrote it in a more mathematical shape, with many alterations of detail in the descriptive parts. After his death it was translated into English and published in 1728; a second edition appeared in 1731, in which the portions contained in the third book of the 'Principia' are marked by an asterisk. A Latin edition (presumably Newton's original) appeared also in 1731, of which there is a copy in the library of the Royal Astronomical Society. It is included in Horsley's edition of the Works of Newton (1782), and in the edition of his 'Opuscula' published at Lausanne and Geneva in 1744. Wolfers added a German translation of the "De Mundi Systemate" in the 'Opuscula' to his translation of the 'Principia,' which appeared at Berlin in 1872. Apparently for the purpose of distinguishing them, he rendered the Latin title of the third book of the 'Principia' "Vom Weltsystem," and of the portion of the 'Opuscula' referred to, "Ueber das Weltsystem."

In the first English translation (1728) of this treatise the comet observed by Newton is called (as in the printed Latin original) that of 1678. But in the second edition (1731) the editor would seem to have noticed an error and made a very unfortunate attempt to correct it, for he calls it the comet of 1668, altering the date of its observation to ten years, instead of one year, earlier.

Yours faithfully,

Blackheath, 1900, Mar. 1. W. T. LYNN.

Periodic Disturbances in the Northern Hemisphere of Jupiter.

GENTLEMEN,—

In the *Monthly Notices*, vol. lix. p. 76, there is an important paper by Mr. W. F. Denning dealing with the subject of periodically recurrent disturbance in the region of the north temperate belt of Jupiter. In 1880, and again in the years 1891–92, there occurred great outbreaks of spots on this belt, perhaps the most remarkable fact connected with them being the enormous velocity of the spots relative to other markings, this velocity being very considerably greater than that of the great equatorial current. In the paper above cited Mr. Denning discusses some previous occasions upon which spots had been observed in former years in this region, and comes to the conclusion that outbreaks of the kind recur at intervals of a little more than 10 years, and calls the attention of observers to the importance of examining Jupiter in February 1901, in order to ascertain if there is any trace visible of a similar outbreak of spots.

It seems to me, however, that the evidence in favour of this period is not very strong, and that, if there is a periodical disturbance here, the period is more likely to be about 12 years than about 10 years. The north temperate belt is seldom or never entirely free from spots, so that the mere fact of spots having been shown by certain early observations and drawings really means very little in itself. Then the circumstances connected with the remarkable oblique streak observed in 1860, upon which Mr. Denning appears to lay much stress, seem strongly to indicate that the phenomena in this case originated at the northern boundary of the great equatorial current, and not in the region of the north temperate belt at all. On the assumption of some eruption giving rise to the formation of a dark spot just to the north of the north edge of the swift equatorial current, and of this spot becoming partly entangled in the latter, the natural result would be for the material of the spot to be drawn rapidly out into an oblique streak, exactly as was observed in 1860.

It would appear, therefore, that we have only two real, indisputable facts to go upon with regard to periodicity, namely, that an outbreak of swiftly moving spots occurred in 1880, and another one in 1891–92. So that if there should be any regular recurrence of the phenomenon in question, which I believe will turn out to be the case, the true period is more likely to be nearer 12 years than 10 years. It is therefore highly important that observers should keep a close watch on the region of the north temperate belt during the next few years. According to my views, the next eruption of these swiftly moving spots is not likely to occur before 1903, whilst, according to Mr. Denning's period of about 10 years, some traces of the outbreak should be apparent in February of next year, or even, perhaps, during the present year.

Yours faithfully,

Hove, 1900, Mar. 14. A. STANLEY WILLIAMS.

Remarks re *Coming Eclipse.*

GENTLEMEN,

It has occurred to me that there are three points concerning the total eclipse now occupying our minds that are worth noting:—

1. Curiously enough, the well-known computer Hallashka has

described it as annular. It is mentioned that it would be total in *Monthly Notices R. A. S.*, December 1872, p. 98.

Also in a list of all solar eclipses visible in England from 1766 to A.D. 2000, computed by Mr. William Chapman, of Foxton, Leicester, this one of 1900 is omitted. Perhaps some reader can give an account of this computer.

2. The well-known eclipse of Thales, 585 B.C., was on May 28 (modern reckoning), and the line of totality seems to have crossed Spain. Those who pay attention to coincidences might regard this.

3. While this is the third of four total eclipses in Spain between 1860 and 1905, a period of only 45 years, I question whether another will cross that country for 300 years to come. (Eclipse of 1842 was also total in Spain, soon after sunrise.)

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage, Bridport,
1900, Mar. 10.

P.S.—One would almost feel disposed to quote from *Punch*, preface, December 1870, at a time when we were expecting the last Spanish eclipse. There is a comic representation of Mademoiselle Selene eclipsing Monsieur Helios. The following occurs on page iv:—"'This is the way I am going to put him out to-morrow,' said the Goddess, interposing her fair round face between her brother's and the Philosopher's. 'You'll be thirty years older, dear clever boy, before I do it again' [referring to the coming eclipse of May 1900]. 'Do you mean to say,' said Diana, reproachfully, 'that, with a grand eclipse to behold, you care about Emperors of Germany, balloon-ministers, battles and sieges, broken treaties, and rubbish of that kind? Who will be thinking of them when the next eclipse happens?'"

Unfortunately, events in South Africa have proved that, as to "battles and sieges," the above was not quite a safe prophecy.

The curious expression "balloon-ministers" refers, no doubt, to the escape of M. Gambetta from Paris in M. Nadin's balloon.

Comet III. 1759.

GENTLEMEN,—

I am doubtful as to Mr. Lynn's solution of the difficulty relative to this comet. Certainly *The Annual Register* leads me to conclude that the observation made on January 9, 1760 (apparently by Mr. Dunn), was a second observation of a comet which he had previously discovered, and it is hardly possible that he could have observed "a near appulse" of the comet to "the star in Orion's right knee" (viz., I believe, κ Orionis) and a passage near μ and ν Eridani during a watch of four hours on the same night. And the comet is, moreover, described as having passed near these stars in Eridanus "in the evening." The only alternative to my previous suggestion, that the Editor mistook Dunn's 7 for a 1, seems to be that Dunn in his note mentioned the day of the week only, and that the Editor dated the discovery a week too early. On one alternative Dunn's discovery was made on the same night as that at Lisbon, while on the other it was made on the 8th, a day previous to the other English observers mentioned by Mr. Lynn. Dunn's observation ought not, I think, to be neglected in computing the comet's orbit.

Truly yours,

Dublin, 1900, March 3. W. H. S. MONCK.

The Magnetic Disturbance of January 19.

GENTLEMEN,—

In connection with your paragraph on page 108 recording a magnetic disturbance of an unusual character at 6 P.M. on January 19 last, perhaps the following may be of interest.

There were no spots on the Sun visible with a 2-inch telescope on January 19, 20, 21, 22, or 23, nor on the 24th with a 1¼-inch telescope. I was away from home, and had only a 1¼-inch telescope with me from the 23rd until the 29th, through which, however, a small spot was visible on the 25th, its position being (as nearly as I could determine by the rough means available) long. 46°, lat. −7°. This spot was still visible on the 26th and 27th, but not on the 29th, and as it was at best but a very small spot, and appears to have burst up on the visible hemisphere on January 24th or 25th, it is most improbable that it had any connection with the magnetic disturbance.

But at 1.15 P.M. G.M.T. on January 27 a much larger spot was seen with the 1¼-inch, just inside the east limb, having apparently come round from the further side. This spot was again seen on the 29th; and on the 30th, with a 5-inch telescope, I found its position to be approximately long. 347°, lat. +4½°. The longitude of the Sun's central meridian at the time of the magnetic disturbance on January 19 was approximately 154°, that of the centre of the further hemisphere being therefore 334°, so that the spot in question, if it existed at that time, was 13° past the central meridian on the further hemisphere.

By the 5th of February the spot appeared to have lost its activity, the north-to-south bridge having then become penumbral, and being the only bridge remaining. The spot was still visible on February 7, it being then within 11° of the western limb.

Yours sincerely,

Madeira, 1900, Feb. 16. WM. ANDERSON.

PUBLICATIONS.

Prof. Burnham's Catalogue of Double Stars *.—Double-star work can very conveniently be arranged under the three headings of Discovery, Measurement, and Computation. To each branch in turn has Professor Burnham given his attention to its great advantage. It is, however, as a discoverer of difficult and interesting double stars that he is most widely known. His discoveries, extending over a period of 25 years, have been published in 19 catalogues, scattered through the *Memoirs R. A. S.*, the *Monthly Notices R. A. S.*, the *Astronomische Nachrichten*, the Lick Publications, and the Publications of the Washburn Observatory. In all there are 1274 pairs, and it was evident that sooner or later a general catalogue would have to be formed. Further, it was not to be expected that observers would, each for himself, collect and tabulate the measures of these stars. To prevent the indiscriminate observing this would lead to, and to direct the work into the proper channel, something beyond a mere catalogue was requisite. It is with great pleasure we are able to announce to astronomers in general, and to double-star observers in particular, that, through the liberality of Miss Bruce, Prof. Burnham has been enabled to give us this desideratum, which is at once a source of reference for the latter, and a means of enabling the former to get some conception of the life-work of this great observer.

The catalogue is issued as the first volume of the 'Publications of the Yerkes Observatory,' and is in size, appearance, and internal arrangement a facsimile of the Lick volume which contains so many of Burnham's measures and notes. The Yerkes Observatory is thus to be congratulated on securing such a standard work for its maiden issue. We have always held Prof. Burnham in high esteem, but could he advance further in our regard it would be through the extreme appropriateness of the Dedication—

To the Memory of
Baron Dembowski
THIS VOLUME
IS GRATEFULLY INSCRIBED.

To the 1274 pairs already published, a few more have been added, and the whole 1290 stars are arranged in order of R.A.; the complete measures given, with references suffixed to each star; and notes, diagrams, and orbits are added. We have thus a complete history of each pair. Naturally the Introduction is of a most complete character, and much of it is of very general interest. Here, for instance, is an analysis of the catalogue, showing at a glance the instrument used, the locality, and the discoveries made at each :—

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* 'A General Catalogue of Double-Stars discovered by S. W. Burnham during the Years 1871 to 1899.' University of Chicago Press, 1900.

6 inch	....	Private Observatory	....	451
18½ ,,	....	Dearborn ,,	....	413
36 ,,	....	Lick ,,	....	198
15½ ,,	....	Washburn ,,	....	87
12 ,,	....	Lick ,,	....	56
9·4 ,,	....	Dartmouth ,,	....	24
26 ,,	....	Naval ,,	....	14
40 ,,	....	Yerkes ,,	....	8
16 ,,	....	Warner ,,	....	2

This table is supplemented by photographs of the 40-inch Yerkes refractor, the 36-inch Lick, the 18½-inch Dearborn, and the 6-inch Clark. Surely this also points to a unique experience in telescopes and climates.

On another page we find "My attention, for some reason or other, which I am unable to explain, had been almost exclusively directed to double stars. This preference was not in any sense a matter of judgment as to the most desirable or profitable department of astronomical work, or the result of any special deliberation upon the subject. It came about naturally, without any effort or direction upon my part." Just so, and when we look at the result *we* cannot cavil at nature, and *he* must feel gratified. It is no small thing to have discovered a stellar system whose components complete a revolution in the short span of life allotted to man. In this catalogue we have at least five systems,

β 989, κ Pegasi,	of	11·4 year	period,
β 883,	,,	15·8	,,
β 101, 9 Argûs,	,,	23·3	,,
β 733, 85 Pegasi,	,,	25·7	,,
β 151, β Delphini,	,,	26·7	,,

not only fulfilling such condition, but they have actually completed their period under Prof. Burnham's own observation. There are, in addition, 180 certain binaries for some of which orbits have been already computed.

Another feature is that of the 1290 stars there are 300 visible to the naked eye. There is a general opinion, and a well-founded one, that the Burnham pairs are difficult; hence this catalogue is, in the main, a record of micrometric work done with the large refractors throughout the world. It is thus at once a monument to the skilful observer and the optician. Alvan Clark lived to see the result of his labours, may Prof. Burnham be spared for yet other triumphs. T. L.

NOTES.

Comet Notes.—A considerable number of observations of Giacobini's Comet (*a* 1900) was obtained in the latter half of February, but it was a very faint object of the 13th magnitude, about 1′ in diameter, with a barely perceptible nucleus.

Herr Berberich has deduced the following elements from observations on Feb. 3, 17, 22 (*Ast. Nach.* 3627):—

T ……	1900 April 28·9421, Berlin M.T.	
ω ……	24° 21′ 27″	1900·0.
☊ ……	40 22 31	
i ……	146 27 10	
log *q* ….	0·12446	

By the end of March the comet will be too near the Sun for observation, but it will reappear about a month later, and remain visible for several months. The following is an ephemeris for Berlin midnight, the brightness being given in terms of that at the date of discovery, Jan. 31:—

	R.A.	N. Dec.	Brightness.
	h m s	° ′	
April 27……	1 33 12	17 28	0·97
May 29……	1 9 0	27 7	1·24
June 30……	23 46 54	41 31	1·85
Aug. 1……	19 36 18	41 20	1·76
Sept. 2……	17 39 12	17 50	0·55
Oct. 4……	17 23 12	5 55	0·19

The comet will be brightest in the middle of July, when its brightness will be just twice that at discovery, but even then it will be a very faint object. A. C. D. C.

Minor Planet Notes.—A planet, FB, was discovered by Palisa on March 1; it proves, however, not to be new, but identical with (117) Lomia.

The interesting planet (434) Hungaria was detected by Palisa on its return to opposition, its R.A. being 43^s greater than that given by Berberich's ephemeris in *Ast. Nach.* 3624. A. C. D. C.

Obituary.—A notice of the late Mr. G. J. Symons appears on another page.

Dr. William Marcet, Mr. Edward Joseph Lowe. Meteorology has lost two other supporters by the death of Dr. Marcet, F.R.S., which occurred at Luxor on March 4, at the age of 72 years, and of Mr. Lowe, F.R.S., of Chepstow, who has recently died, aged 74. Dr. Marcet was a physician, and for some time was on the staff of the Brompton and Westminster Hospitals, but was also a student of meteorology, and, besides many memoirs on medical subjects, made two contributions to the literature of that science—one on the weather at Cannes during the season 1875–76, the other a book of some four hundred pages on the 'Principal Southern and Swiss Health Resorts.' Dr. Marcet was elected President of the Royal Meteorological Society in 1888.

Mr. Lowe was one of the founders of the Meteorological Society, and among other memoirs published a daily record of observations made at Chepstow in the years from 1842 to 1882. He was elected a Fellow of the Royal Society in 1867.

M. E. Liais. "The death is announced of M. Emmanuel Liais, who was born at Cherbourg in 1826, became a pupil of Arago and Leverrier, and subsequently held a position at the Paris Observatory. In 1858 he left for South America to observe a solar eclipse, and, remaining in the country, became Director of the Observatory of Rio de Janeiro. He returned to France in 1881, bringing with him his magnificent collection of exotic plants, for which he had conservatories built in a park situated in the centre of Cherbourg. These glasshouses form a colonial garden, and M. Liais has bequeathed them to his native town. He discovered Comet 1860 I., which does not appear to have been observed elsewhere than at Olinda, and was able to disprove the alleged discovery of Lescarbault of an intra-Mercurial planet, as he was observing the Sun at the same time." *

Mr. Leander J. McCormick. The announcement is also made of the death of Mr. L. J. McCormick, of Chicago, whose name will be remembered as that of a famous observatory attached to the University of Virginia, U.S.A. Mr. McCormick had accumulated wealth by the successful invention and manufacture of agricultural machinery, and in 1885 presented the University with the observatory at a cost of £20,000. Mr. McCormick died at the age of eighty-one.

Dr. G. F. W. Rümker. We regret also to announce the death of Dr. Rümker, Director of the Municipal Observatory, Hamburg. This observatory, almost entirely from its foundation, has been in the care of an astronomer of this name. By the aid of money bequeathed for the purpose, the Astronomical Observatory was established by the elder Repsold in 1823, and on his death in 1830 Carl Rümker, who had been in charge of Sir T. Brisbane's Observatory, Paramatta, and had then returned to Germany, was chosen to succeed him. A son, the subject of the present notice, was born to Rümker on 1832 Dec. 31. George Rümker, after his early education, worked for some time at the Berlin Observatory, and then came to England, where he was Assistant to Dr. Chevallier at the Durham Observatory from 1853 to 1855. When Airy undertook the Harton Colliery pendulum experiment in 1854 he collected a band of observers from various English observatories, and George Rümker was one of these. Rümker returned to Hamburg and acted as Assistant at the Observatory until 1862, when his father died and his son was chosen to succeed him. The purely astronomical work of the Observatory under his directorship has consisted of observations of small planets and comets, which have been published in the *Astronomische Nachrichten*, but Rümker has chiefly devoted his energies to the work of regulation of chronometers and subjects connected with navigation. The German Maritime Observatory (Deutsche Seewarte) was established in 1867 at Hamburg, and this rather diminished the importance of the Municipal Observatory; but one division of the work, that devoted to chronometers, which is on much the same lines as at Greenwich, was placed in the charge of the Director of the Municipal Observatory, and this has been energetically carried on by George Rümker.

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* From the *English Mechanic*, March 16.

Prof. PIAZZI SMYTH. We have to supplement the notice in our last number of the late Professor by some information kindly communicated to us since by Mrs. Baden-Powell.

Prof. Smyth died on February 21 at Ripon after a few days' illness; he had been working nearly to the last at photographic spectroscopy. He left no family; his wife, whom he married on 1855 December 24, died 1896 March 24. His elder brother was Sir Warington Smyth, his younger brother Sir Henry Smyth, and his younger sister married Sir William Flower.

Dr. WILHELM LUTHER writes:—"In the last number of the *Observatory* the date of the death of my father is not correct. It was on the morning of February 15 that he deceased very unexpectedly, after having still made astronomical computations on February 10."

THE TOTAL SOLAR ECLIPSE OF MAY 28.—So far as is known at present, the arrangement of British observing parties is as follows:—

Official Expeditions.

Ovar.—The Astronomer Royal will take large scale photographs of the corona, using the Thompson 9-inch object-glass and telephoto concave magnifier to obtain photographs on the scale of 4 inches to the Sun's diameter on 15 by 15 inch plates. The double tube with a 4-inch rapid rectilinear lens of 33-inch focus and a special rapid lens of about 13 inches focus will be used in an attempt to obtain photographs of long extensions of the corona. Mr. Dyson will have in his charge two slit spectroscopes belonging to Capt. Hills and used by him in India in 1898, with which to photograph the spectrum of the flash and of the corona.

Alicante.—Sir Norman Lockyer, who will probably be accompanied by Mr. Fowler and Dr. Lockyer, will be stationed near the central line south of Alicante. The chief instruments to be used here are prismatic cameras, one of which will have an aperture of 6 inches and a focal length of 20 feet.

Algiers.—Prof. Turner and Mr. Newall will be stationed at the Observatory of which M. Trépied is Director. Prof. Turner will have one of the double tubes which have been used in several previous eclipses. One section will be fitted with apparatus for photographing the corona by polarized light, the other will possibly be utilized for taking ordinary photographs of the corona. Mr. Newall will use: (A) a four-prism slit spectroscope for photographing the spectrum (i) of the flash, (ii) of the corona; (B) an objective grating camera for photographing the corona in monochromatic light; (C) a polariscopic camera for photographing the corona.

Prof. C. V. Boys and Mr. Wesley will probably be also members of the official party at Algiers.

Mr. Evershed will proceed to a spot about 20 miles south of Algiers, which is just within the limit of totality. This place is chosen as it is his purpose to obtain photographs of the "flash" spectrum of as long duration as possible, and near the Sun's pole.

British Astronomical Association Expeditions.

Algiers.—Mr. and Mrs. Maunder and family will take short exposure photographs of the inner corona, and will also take photographs of long exposure for extensions. A prismatic opera-glass will be used, if circumstances allow. Mr. and Mrs. Crommelin will use a tele-photo lens of about 80 inches equivalent focal length and aperture 1½ inches to take photographs of the inner corona. They will also observe times of contact, and make sketches at the telescope of detail of the inner corona.

SEARCH FOR AN INTRA-MERCURIAL PLANET.—The Harvard College Observatory authorities intend to devote their resources during the coming total eclipse to a search by means of photography for a planet within the orbit of Mercury. The plan proposed by Prof. W. H. Pickering is published in Harvard College Circular No. 48, dated 1900, Feb. 13, where it is stated that it has been shown that the photographic intensity of the light of the sky round the Sun at the time of a total solar eclipse is about equivalent to that of the region of the pole-star, three minutes after that star first becomes visible to the naked eye in the evening; and secondly, the faintness of a star that may be photographed against a bright background of sky depends within limits on the length of focus of the lens and not on the aperture.

Starting with these facts, experiments have been made, and the most suitable telescopes for the undertaking have been found to be of 3 inches aperture and 11 feet 4 inches focal length. A battery of these telescopes is to be provided which will command the sky for a length of sixteen degrees on either side of the Sun, the breadth of the zone covered being ten degrees throughout its length. It is considered reasonable to suppose from certain analogies that an intra-Mercurial planet, if such exists, would move in a plane not far from that of the solar equator; and as the Earth passes through this plane about one week after the eclipse, the occasion is thought to be a favourable one for the search. It is proposed to expose these plates somewhere in the State of Alabama, and the suggestion is made that a similar apparatus should be used in Spain or Algeria, so that in case such an object should be found an approximate orbit might be computed.

Prof. Pickering begins his circular by explaining that it is not in general the policy of the observatory to observe solar eclipses, for, even if it is clear, "the results in many cases are only a series of pictures of the corona and protuberances, which add but little to our knowledge of them." This seems to be a little bit severe on other astronomers.

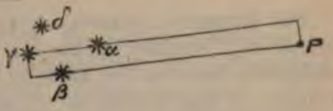
The Colour of the Background of the Sky.—Dr. T. J. J. See makes a suggestion in *Ast. Nach.* 3628, which seems worth noting. He refers to the difference of shade of colour of the background of the field when a telescope is swept over the sky, which has probably been noticed by many persons. He says that he particularly noticed this when he was making the survey of Southern Double Stars at the Lowell Observatory, and that on nights when the seeing was perfect, the atmosphere dry and quiescent, and there was no Moon, the sky appeared in patches to be of a brownish colour, and suggests that this colour owes its existence to immense cosmical clouds which are shining by an excessively feeble light. One sentence of Dr. See's, which states that this was seen in fields devoid of stars, suggests another possible cause, namely, that the excessive blackness of fields containing bright stars is due to contrast with the starlight, and other fields may appear brown in comparison with these; but this suggestion is negatived by Dr. See's statement that certain fields, such as the Coal-sack, which contain no stars, appeared perfectly black. Dr. See states that these brown patches which he has observed cluster especially in certain regions of the Milky Way.

The Probable Number of Telescopic Meteors.—In the same publication Dr. See refers to another subject brought to his notice when working at Lowell Observatory. He says that in sweeping across the sky with the 24-inch telescope, which had a field of 6 diameter, it frequently happened that a telescopic meteor was see to dart across the field. A careful consideration of his whol experience leads to an estimate of not less than five meteors pe night. Assuming that the average length of a trail is one degre and with the above size of field and estimated number as dat

Dr. See concludes that 1200 million telescopic meteors are added every 24 hours to our atmosphere. Dr. See recalls Prof. H. A. Newton's conclusion that from 10 to 25 million naked-eye meteors are added to our globe every day of the year.

A Method for Finding the South Pole of the Heavens.—The connection between the war in South Africa and astronomy is not at once apparent, but two matters in which the subjects are associated have lately been brought to our notice. The first is a mistake of a journalist who, describing the surrender of Cronje on the morning of February 27 (it will be remembered that the new Moon happened about noon on March 1), writes of the *young* Moon rising at that period, and that after it had pursued its course for about two hours its *light showed* the Boer Commander that he was surrounded, &c., &c. The second matter, on the other hand, is not about a mistake, but supplies useful instruction in naked-eye astronomy. It occurred to a certain public-spirited gentleman, a Fellow of New College, Oxford, that as it is often necessary to know the cardinal points in South Africa, and compasses are not always trustworthy owing to the magnetic nature of the rocks in that country, it would be well to supply to those whom it might concern an easy method of finding the South Pole. He therefore applied to the Savilian Professor of Astronomy, who, with his usual readiness to help, arranged the following plan, which the aforesaid Fellow has had printed for private circulation. The following is printed on a slip of paper $14\frac{1}{2}$ inches by 1 inch :—

> To find the South Pole. Hold this paper (or, better, a wooden ruler of the same size, $14\frac{1}{2} \times 1$ in.) at 24 inches from one eye, closing the other. (This is approximately the full length of both arms; but, as arms vary in length, the right distance should be found by trial.) Adjust one edge on the axis of the Southern Cross, as shown in the diagam, the head of the Cross being at the corner, and see that the other edge passes through the *brighter* of the two stars forming the cross-piece. The South Pole lies at the corner of this edge, P. This holds good for all positions in which the Cross may be lying.



A supposed Connection between Solar Activity and the Position of the Ecliptic.—A remarkable paper by Dr. J. Halm, of the Royal Observatory, Edinburgh, has appeared in *Ast. Nach.* No. 3619. It can scarcely be said that his conclusions are definitely established, so that the following summary of the paper must not be considered as endorsement of Dr. Halm's suggestion.

It is common knowledge that though the period of the sun-spot cycle is usually stated to be about eleven years, its actual length varies considerably from this mean value. It has been suggested, but not by any means proved, that the sun-spots conform to a combination of two periods, the longer of which is about sixty years.

Dr. Halm starts with this as a fact, and states that the sixty-year period had a minimum about the middle of the 18th century, a high maximum in 1783, a low minimum in 1816, and again a maximum in 1838, and so on.

The author then proceeds to show that this period has a counterpart in certain movements of the Earth's orbit. He takes the observed values of the obliquity of the ecliptic from the Greenwich observations from 1753 to 1896, and applies corrections to reduce them to a common standard, and compares them with Leverrier's formula. He then tabulates the differences and takes means of each consecutive twelve: these means being plotted to form a curve show first a gradual increase with the time, which might not have existed if the observations had been treated differently, but this point is not material to the argument; and next the curve shows variations from this mean straight line of increase, which have maxima about the years 1780 and 1843, and minima about 1816, 1860, and 1888. These coincidences with dates of similar phases of the long sun-spot curve are taken as establishing a connection between the phenomena.

Certainly, says Dr. Halm, if there is a relation between sun-spots and the obliquity, the eleven-year sun-spot period should be represented in the figures giving values of the obliquity, and he therefore proceeds to apply corrections to the differences above described to eliminate the variations of long period which he suggests. The resulting curve consists of waves of semi-amplitude about $0''\cdot5$, but it is difficult to establish a similarity between these and the waves of the spot cycle. Dr. Halm goes on to show that these are a result of the variation of latitude, but, as we have said, it is difficult to see a connection with solar phenomena.

THE RUSSIAN CALENDAR.—The fact that the non-occurrence of a 29th of February in this year has had the effect of making the difference between the Russian Calendar and that of the Western Church 13 days instead of 12 seems to have brought the question of change of style under the consideration of the Czar. A short time ago a commission was appointed to elaborate a new calendar, and the result of their labours has recently been explained by Prof. Glasenapp to the Russian Astronomical Society.

Exact description is not to hand, but it is stated in a note in the *Times* of March 16 that the proposal is to adopt neither the Julian nor the Gregorian system, but that the new system is such that it will not be more than a day in error in 100,000 years. This may be the same scheme as that proposed by Mr. Lynn in *Nature* (1896 June 11), according to which every 128th year would not be a leap-year, so that of 128 years 97 would be common years and 31 would be leap-years, which would make the average length of a calendar year 365·24219 days; and as the length of a tropical year is 365·24220 days, the error would only be 0·00001 day in 128 years.

THE ORBIT OF τ CYGNI.—Dr. T. J. See publishes in *Ast. Nach.* 3629 an orbit of τ Cygni (A.G.C. 13). He deduces a period of 57·25 years, and a value for semi-major axis 1″·16, and gives the following ephemeris:—

	°	″
1900·7	307·0	0·84
1901·7	301·3	0·82
1902·7	294·9	0·81
1903·7	288·6	0·80
1904·7	282·0	0·80

In the same issue he gives the measures of 95 Ceti, and points out that the period is about 150 years.

WE have received from Mr. Stanford, of Charing Cross, copies of the new editions of Mr. Lynn's useful little books: the fifth edition of 'Remarkable Eclipses' and the eighth of 'Remarkable Comets.' We have expressed our appreciation of the excellence of these booklets and their handiness so often that we need say little more in their praise, but it may be well to describe to those of our readers who have not seen them, the scope of these works. The eclipse book gives a very plainly written sketch of the most interesting circumstances connected with solar and lunar eclipses in ancient and modern times: the new edition contains the salient facts of the coming eclipses of this and next year. 'Remarkable Comets' gives similar descriptions of the most interesting of these celestial bodies, and gives among other valuable information a list of all the comets which may with some confidence be expected to reappear. The price of each is sixpence, and they can be carried with ease in a coat-pocket.

OUR best congratulations to Mr. W. E. Plummer, Director of the Bidston Observatory, Liverpool, on his election to an Honorary Readership in Astronomy at University College, Liverpool. It is to be regretted that this post is honorary, but it was bestowed on Mr. Plummer by the Senate of the University as the only recognition they could make of his services in lecturing on Astronomy in the University, which he has given gratuitously for some time. The resolution of the Senate runs as follows:—

> That in recognition of his astronomical learning, and of his skill and ability to teach the subject, Mr. W. E. Plummer, M.A., F.R.A.S., Director of the Bidston Observatory, be appointed Honorary Reader in Astronomy in University College, Liverpool.

THE Royal Meteorological Society will celebrate its Jubilee on April 3. A commemoration meeting will be held on that day at the Institution of Civil Engineers, and a Conversazione in the evening at the Royal Institute of Painters in Water-Colours, Piccadilly. On the following day the Astronomer Royal will receive the Fellows at the Royal Observatory, Greenwich.

At the Meeting on February 21 Dr. C. Theodore Williams was appointed President for the ensuing year, in place of the late Mr. G. J. Symons, the newly-elected President, who was then suffering from the illness from which he died.

The Isaac Newton Studentship at Cambridge has been awarded to J. Hopwood Jeans, B.A., Scholar of Trinity College.

The next Meeting of the Royal Astronomical Society is on Wednesday, April 11. Of the British Astronomical Association on Wednesday, April 25.

FROM AN OXFORD NOTE-BOOK.

As long ago as January 1897 a suggestion was made in these notes which found favour with one reader (possibly others too), who thereupon took up his pen and wrote urging the carrying-out of more than the original suggestion. But after writing he hesitated to send the letter and kept it "sleeping ever since in a remote drawer." I am very glad he has at last repented and let us have his interesting proposal. It is one which has been in the minds of the Editors of this magazine at various times, but they have not seen their way to carry it out. Encouraged, however, by this definite request, they are taking the matter into serious consideration, as I will explain in a moment. But first let me give the letter; it is written by a foreigner, as will be noticed from one or two little quaintnesses of style, which I trust the author will forgive my leaving untouched, for they seem to me to add to the reality of the letter :—

> The regret of only knowing the biography of astronomers (and generally of eminent contemporaries) when they are dead, has been long since my feeling, and I cannot but think very advisable the suggestion you make about that matter. But it is also a pity that the portraits are likewise published only when the originals are no more. As a means of preserving a lasting memory of their features and even of themselves, it is quite right, but for those who long to know how look some leading astronomers whose work is daily forcing itself to attention and wonder, it affords no mean of satisfying that legitimate curiosity. One may understand better the character of anyone's work by catching from his face the glimpse of genius. Now, in this age of illustrated newspapers, the astronomical reviews ought to start the publication of the portraits of living astronomers, as soon as known to fame, or nominated for some conspicuous standing. Would not such a scheme for an astronomical gallery be welcome throughout the science world?

Now there are two difficulties in the way of carrying out this scheme. The first and most important is financial. I am no longer an Editor of this magazine, but having been one for some years I know all about the finances, and can speak more freely than an actual Editor. The magazine just about pays its way at present; there is no loss, but there is no appreciable profit. When a few pounds appear on the credit side the Editors generally give a "picture," which soon wipes off the balance. An extensive series of photographs would be costly, especially if well done, and it does not seem likely that any of the expense would be recovered as in the case of the publication of other photographs. The Royal Astronomical Society has initiated very successfully a system of publishing photographs which pays its way. The Photographic Committee (which means Mr. Maw and Capt. Hills) was started by the Council with £20; but the sales have been so good as to allow of this sum now being returned to the general funds. With portraits, however, it would be different; there might be a small sale of extra copies, but it is very unlikely that these sales would be sufficient to meet the outlay. Hence it is necessary to consider the project as a costly one. The cost per portrait cannot well be less than £5, and taking 20 as the minimum number of portraits which would be welcome, £100 is the least sum which would give it a fair start. If one of our generous friends of astronomy would provide a sum of this kind in aid, I cannot help feeling that he would spend the money to excellent purpose. Failing substantial aid of this kind (or perhaps supplementing it), does the project commend itself sufficiently to justify a subscription list? I hereby signify my willingness to contribute £5, and if 999 other gentlemen will do the same we shall get along famously. Even 19 would be a help. I should add that, though the Editors of this magazine allow me to make this suggestion, they are anxious to make it clear that subscriptions would not be a subsidy of the magazine itself. Accounts of the "Portrait Fund," if it can be started, will be kept quite separate and published at the end of the year. The second difficulty (a minor one) is the selection of portraits, which is a delicate matter. The Editors must be left to deal with it, but they would doubtless be grateful of assistance in selecting; and if as many readers as possible would send a list of (say) 20 names which they would be glad to see represented pictorially, it would be a great help. Of course these lists would be entirely private.

An incident of a trivial kind is sometimes suggestive. The other day I had consulted a recent volume of the Greenwich Observations on some point, and, having finished with it, wished to return it to its place in the series, either of my own private copies or of the Observatory library, I was not sure which. A computer was told to ascertain whether its place in the Observatory library was vacant, and if so to put it there; but he was given his choice of two courses—he might visit the library first, returning for the book if the place were vacant, or he might take the book with him. He at once chose the former, though it involved a double journey. The volumes have indeed swelled to huge proportions, and are quite an item in the way of weight; but it is proposed to cut them down considerably by only giving details for the fundamental stars, and the change will be welcome. There is an old story about Pogson (perhaps the present Director at Madras, or rather Kodaikanal, can tell us if it is true?) that once when he took down a volume of the Greenwich Observations, a snake came out! (I don't know whether it had made a nest among the transits: I only tell the story as it was told me.) There would be room for several snakes in the present volumes.

THERE are many who will be glad to learn that Prof. Barnard, who recently had the misfortune to break a small bone in his hand while crossing London in a hansom, through the horse falling down, has reached Williams Bay again without further mishap. A letter dated March [illegible] reports that Prof. and Mrs. Barnard are well, though the injured hand is not yet out of splints. On their return to Wisconsin they found the country covered with snow and ice, although the winter had been "quite mild"; the lake was frozen over with some [illegible] inches of ice." Prof. Hale is very busy getting ready for the eclipse, and his new spectroheliograph works admirably, and promises to be far the best yet made.

A CORRESPONDENT sends us the following "startling intelligence" contained in the *Scotsman* [illegible] March [illegible]:—

The Loss of the Planet Mercury.

On Saturday a letter was received in Dundee from the owners of the Planet Mercury (Messrs. R. W. Leyland & Co., Liverpool), which stated that the underwriters had had intimation that a [illegible] belonging to the Planet Mercury had been washed up on the coast of Nova Scotia.

It is not often that an astronomical question is asked in the House of Commons; but on Thursday, March [illegible] last, the Moon was called to order. The account is given in *Punch* in the following words:—

Thursday.—"A calendar, a calendar! Look in the almanack. Find out moonshine, find out moonshine." Thus Bottom.

Richardson, [illegible] made a painful discovery. When George the Second was king he ordained, [illegible] that Easter Day should be the first Sunday after full Moon which happened upon or next after the [illegible] March. [illegible] George the Second—the full Moon [illegible] a Sunday, Easter Day should be the Sunday after.

[illegible] Richardson, having looked in the almanack, finds that the first full Moon happening [illegible] after the [illegible] day of March current finally presents itself two minutes after one o'clock in the morning of Sunday the [illegible] of April. [illegible] Easter Day should be the [illegible] of April, not the [illegible], as the calendar decrees.

Here's a [illegible]. What is to be done to avoid catastrophe? Richardson [illegible] Government's [illegible] Many men would have [illegible]. Most men would have suggested that the question should be addressed [illegible] the First Lord of the Treasury. [illegible] the Moon [illegible] the case, and found she was [illegible] he said confidentially to the Speaker, "The full Moon referred to in the statute is not the actual full Moon, nor the mean Moon, but a fictitious and arbitrary [illegible] Moon, sometimes called the ecclesiastical full Moon."

Uneasy feeling that this is ribaldry. But Attorney-General may be trusted to know what he's about. House fully reassured, happy in possession of an extra Moon whose existence was hitherto unsuspected, turned with light heart to sublunary affairs.

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Wednesday, 1900 April 11.

Mr. E. B. Knobel, *President,* in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Dyson. 78 presents have been received since the date of the last Meeting. Of these the following may be mentioned specially. From the Lund Observatory we have received a copy of the observations made for the Astronomische Gesellschaft Catalogue, Zone +35° to +40°. From the American Nautical Almanac Office we have received the Ephemeris for 1900. We have also received the Volume of Greenwich Observations for 1897; the Cape of Good Hope Catalogue of Zodiacal Stars for epoch 1900; a reference Catalogue of Southern Double Stars compiled by Mr. Innes, published as an appendix to the Cape of Good Hope Observations; Mr. H. L. Rice of the American Nautical Almanac Office has sent us a copy of his new book called 'Theory and Practice of Interpolation'; we have also a memoir by Prof. Schiaparelli on his observations of Mars in 1888; a volume from the Toulouse Observatory; a volume of Double Star Measures from Prof. Glasenapp; three volumes of Observations from the Harvard College Observatory. There has also just been received an enlargement of a negative taken by Mr. C. P. Butler on 23rd November 1899, of an exploding meteor, which shows both the explosion and the change of the track after the explosion. The Royal Meteorological Society has presented us with a specimen of their Jubilee Medal, and the Paris Observatory has sent us a second instalment, 24 charts, of their enlarged copies of Astrographic Chart Plates.

The President. In asking you to return your thanks to the donors of these presents, I would like to make a remark upon one present that we have received, namely the 6th part of the Observations of Mars by Schiaparelli. From a recent letter I have had from Prof. Schiaparelli I learn that there will be one further publication of his observations of the Planet Mars, and that will be the last. The Society will regret to hear that owing to failing eyesight his observing career is coming to an end.

The President called upon Mr. Butler to explain the photograph which he had presented to the Society.

Mr. Butler said that the original photograph was taken with an ordinary camera placed on a window-sill, on the night of 1899, Nov. 23, so that it was taken with practically instantaneous exposure. The enlargement was 81 diameters. The meteor was seen to fall by others, though not by himself. As a matter of fact, he put the camera on the window-sill and went to bed, got up before it was light and shut the camera up. On the Monday morning following he developed the plate, with the result shown in the photograph.

A Fellow. What was the lens you were using?

Mr. Butler. A triple achromatic one from an opera-glass. The photograph was taken on an ordinary slow plate. Though we, at South Kensington, have been watching for meteors systematically for the last six years, and I and others have worked for several hours each night, using 20 plates a night, we have never got a single trail, except this, which was got accidentally.

The President. I am sure you will join with me in congratulating Mr. Butler upon his patience having been rewarded by such an interesting result as this.

Mr. A. Fowler then read a paper on the Effects of Stellar Rotation and Revolution upon the Spectrum lines.

It was pointed out by Sir William Abney in 1877 that the effect upon its spectrum of the rotation of a star would be a broadening of all the lines, if the axis of revolution be not directed towards us. A little later the distribution of intensity in a line broadened in this manner was investigated by Vogel, but spectra satisfying the conditions were then unknown. Some years ago, however, Prof. Pickering suggested that rapid rotation would explain the peculiarities of the spectrum of α Aquilæ, which had been noted in the Harvard photographs, and the same explanation has been adopted by Lockyer and Vogel. In view of the possible application to other peculiar spectra, it has been thought desirable to investigate these effects more completely. If all parts of a star have the same angular velocity, the points having equal component velocities in the line of sight lie in planes perpendicular to the equator and passing through the Earth, the component velocity being proportional to the distance from a parallel plane containing the axis. If the star appears as a uniformly illuminated disc, the intensity of a broadened dark line at any point in its width will be proportional to the chord containing points of corresponding component velocity. Thus, taking the intensity at the middle of the line as unity, it will fade off to zero at the edges, but even at nine-tenths of the distance from the middle the intensity will be as much as 0·44. If the angular velocities are different in different latitudes, as in the Sun, the lines will fade off more rapidly towards the edges. The effect upon bright lines would appear to be different from that upon dark lines. The most interesting case is that in which the bright line is assumed to be produced by a shell of vapour surrounding a spherical nucleus, the effect of rotation being to produce a broadened line with two maxima of intensity.

It does not seem unreasonable to suppose that some of the many spectra with diffuse dark lines, which are now known, may be explained by rotation, especially as the forms of the lines need not be precisely identical in different stars. Again, in stars of the γ Cassiopeiæ class, where the bright lines of hydrogen are double, all the dark lines are invariably wide, so that rotation may perhaps explain the peculiar appearances of both the bright and dark lines.

Mr. Newall. There are undoubtedly a great many cases of the diffuse stellar spectra that require explanation, and something on the lines Mr. Fowler has suggested seems very natural. There is evidently a considerable class of stars in which the spectra show diffuse lines. As Mr. Fowler has pointed out, α Aquilæ is an instance, and one may also mention certain other stars, such as β and δ Leonis, α Trianguli, &c., as similar; and we have a definite set of phenomena that want explanation. Mr. Fowler, following the lines first laid down by Sir William Abney, emphasizes this possible explanation by reference to rotation, and I look forward to studying his paper in greater detail. With respect to the doubling of the bright lines in γ Cassiopeiæ, the thing which strikes me at first sight in his explanation is that he is dealing with a very ideal case, viz., a mass of gas which is supposed to be emitting light at the same rate from every part of the mass of gas—that is, independent of the distance from the centre, and, if I understand it aright, equally transparent throughout the mass. He takes no account of the absorption by superincumbent layers of the light emitted from the layers down below. In any case we must all be glad that Mr. Fowler has got his eye upon this particular set of facts, which certainly does require explanation.

Mr. Crommelin. Mr. Fowler has referred to the case of a star revolving about another. It seems that if a star revolves without rotation, every part is coming to the observer at the same rate, and I do not understand how that will produce a diffuseness in the lines. It seems to me that the effect would be to only shift the lines bodily and not to increase the diffuseness.

Mr. Maunder. I was very glad to hear that Mr. Fowler had taken up this enquiry. It is an exceedingly interesting one, and I was especially pleased with the very pretty explanation of the doubling of the bright lines in stars. I should like, however, to go through the paper in detail, because, from just listening to it, I could not follow the intensity curve, which it seems to me is the crucial matter in the whole problem. It seemed to me, from a first glance at the matter, that the variations in the intensity from the centre of the line to the outside must be very great, so that I did not anticipate that the broadening would be so marked as Mr. Fowler says.

Mr. Seabroke. I did not quite catch whether there was a duplication of the bright lines, and merely a widening of the dark lines. Perhaps Mr. Fowler will refer to that.

Mr. Fowler. In my anxiety not to take up too much of the time of the Meeting, I am afraid I cut my explanations rather too short. As regards Mr. Crommelin's remark that the revolution of a star will not affect the form of the spectrum-lines, it seems to me that in the case of a close system the nearer part of the star must get round the orbit in the same time as the outer part, although it has a shorter distance to go, so that all parts of the star would not be travelling at the same rate. With regard to the apparent duplication of bright lines, as compared with the simple broadening of the dark lines, I would point out that we have a very different distribution of effective vapour in the two cases.

While the dark lines are produced by vapours in front of the star, the bright lines will be chiefly produced by the vapours round the limb, and in the latter case there would be a preponderance of vapour having nearly equal velocities at the approaching and receding limbs.

The President. It is quite clear that this is not only a very interesting subject, but one of some complexity, and we cannot but be grateful to Mr. Fowler for having given so much attention to it, and for having introduced this paper for our consideration.

Prof. Turner. I would like to put on the screen a diagram which may be interesting to those who are going to observe the next eclipse. The result relates to the photographs of 1893, and it gives a general idea of the brightness of the corona. The brightness was visually determined by Dr. Thorpe, in 1886 and in 1893, with very similar results. Dr. Thorpe measured the intrinsic brightness up to about six-tenths of the radius of the Sun from the limb, but not within that, and consequently from this point to the limb we cannot compare photographic results. For the portion where there is comparison, the results are very accordant. Sir William Abney has expressed his surprise that they should be so accordant. Comparing the total visual brightness which *was* measured with the total photographic brightness, I find that the visual brightness must have been even greater near the limb, so that we can imagine the visual curve coming outside and following the photographic curve closely. The total visual brightness was something like the Moon, and in making this curve I have assumed that the Moon is equal to one-fiftieth of a candle at one foot. I am sorry that the Moon has not been photographically measured in the same way through the same apparatus, because we have to take that on trust. As it is, the corona seems to be about half as bright as the Moon near the limb.

With regard to the form of the curve, it follows very nearly the law of the inverse square of the distance from the limb which was proposed by some of the American observers in 1878, and which observers have since rather discredited. I found that by putting the zero of distance just within the Sun's limb, the law of inverse square of distance from that point will be very near the truth.

Now, to come to a different subject, I ventured to trouble the Society some time back with an attempt to explain mathematically the existence of stationary meteoric radiants, for which Mr. Denning had so consistently fought. The method was to take account of the Earth's action on the meteors which passed very close to it, not so close as to be stopped by the Earth, and seen by us, but passing to the right and left of it. If we have a stationary Earth with meteors passing on opposite sides of it, then the effect of the Earth's attraction upon them is to do three things. It first of all bends the paths of the meteors towards one another, and so we get a deflection, but, secondly, it leaves the velocity unchanged after deflection. Thirdly, it has an important effect in quickening the passage past the Earth, because the velocity is greater at every point where the Earth is acting, and so the meteors are pulled past the Earth quicker. Now the change of direction, which is in some respects awkward, may be rendered negligible if the thousands of meteors which pass on opposite sides meet together and destroy the velocity of approach. In the earlier paper I laid most stress upon the alternation of the meteors passing on both sides. I now think that the explanation I gave will not be of much account unless there is some mutual interaction of the meteors. If there is no interaction, and the meteors are to be regarded as a cloud of immaterial points, then I must confess that the general theory is not in a very strong position; but if we may suppose there is any sort of cohesion in the meteor swarm, then this convergence of the meteors may result in some sort of neutralizing of the components of velocity perpendicular to the line of relative motion, which thus remains unchanged in direction; but there is always this important sudden displacement in the direction of the meteors' passage relative to the Earth. Now, if we can grant that displacement without any change of relative velocity or change of direction, then we get the explanation of stationary radiants. The consequence will be for us to meet them next year earlier than before. My idea would be that we get gradually a scattering of the meteor-orbits around the orbit of the Earth, the relative velocity, and therefore the radiant, being preserved intact. The former paper has been criticised by M. T. Brédikhine in Russia, and the present is a reply to his criticism. I think he has mistaken the idea of the paper. He has taken the velocity of the meteors *in space* and carried it bodily round the Earth's orbit, and finds it does not lead to a stationary radiant at all, which is quite true. But that is not my supposition in any way. The paper is firstly a reply to Mr. Brédikhine's criticism on those lines. But I have, secondly, a reply to a much more pertinent criticism by Prof. Newcomb, who did me the honour to talk over the theory at Oxford, and he said:—"Supposing there was an action of this kind, it would require a very long time to develop; a great many encounters of the meteors with the Earth to produce any sensible result. Now before that could happen the general perturbation of the other planets would carry the swarm away from the Earth's orbit, and the particular action of the Earth would not have time to aquire any considerable dimensions." In the paper I give reasons why I think the meteors should continue to pass near the Earth. I think the passage on the opposite sides of the Earth may have a sort of condensing effect on the swarm towards the Earth's orbit.

Supposing that we had a nebula with a bright nucleus, the natural tendency would be to conceive the nebula as condensing towards the nucleus. The condensation might be slow, but it would exist. It would depend upon the inelasticity of the particles. We should get no permanent condensation if the particles never encountered one another. If we assume that these particles meet or affect one another in any way, then it seems to me we should get condensation towards the nucleus. That is not a very unnatural supposition, and if we are allowed once to make it I think we can get the whole of our facts. The same thing would happen if the nucleus were not there always, but were there intermittently, being annihilated in the meantime; or without supposing annihilation we could suppose that the nucleus intermittently passes through the swarm along a certain definite line. In this case the condensation would be, not towards any one point of this line, but towards the line generally, and of course would be still slower.

To go a step further, the same thing would happen, only more slowly still, if the line were not a single line, but a series of lines forming a surface in the nebula. The nebula would then condense towards the surface formed by those lines. That seems to be a fair representation of the Earth passing through the meteor-swarm. It seems to me a not unfair supposition that we should get a condensing of the swarm to the surface in which the Earth's orbit intersects it, and the result would be that the swarm, instead of being diffused, would become a sort of sheet passing through the Earth's orbit. I am afraid the idea is little more than a speculation: and it was undertaken in the first instance with the definite object of seeing whether the fact for which Mr. Denning contended so stoutly could not be explained on theoretical grounds. My chief object was that Mr. Denning's careful observations should not be dismissed by what I am afraid is almost contempt by theoretical astronomers.

But it is not the first occasion on which theoretical astronomers have shown this impatience of observed facts, for which they ultimately found a sound explanation. When Mr. Chandler announced from a study of observations that there was a variation of latitude in a period of 14 months, the idea was scouted as impossible. Euler had shown that the period, if any, was 10 months, and it was some time before theoretical astronomers could be induced to consider a 14 months', when they had been accustomed to a 10 months' period; when, however, they did consider it they found they had made the assumption of the Earth being a rigid body. They were so accustomed to it that they forgot to make allowance for this not being quite true. Now I well know that it is a very difficult thing to suggest anything new in planetary theory which has been worked at so long; I do so with the greatest hesitation; but my chief reason for doing so is that if Mr. Denning's observations are well founded there *must* be some explanation of the existence of stationary radiants, and it is quite likely that a simple cause like the action of the Earth may be the explanation. Just as in the previous case, the action of the Earth on particles or rigid bodies has been duly considered; but a swarm is *not* a rigid body, and the interaction of its parts may cause it to behave very differently from a rigid body. I have shown qualitatively how the action of the Earth on such a swarm may explain stationary radiants; but the quantitative estimation of such action may not be sufficient, that remains to be seen.

The President. It seems to me that the theory now advanced in regard to its operation, which would bring about an increased number of the swarm of meteors, would take a very long time, but the act of producing a stationary radiant, as Mr. Denning has suggested, has not taken a long time.

Prof. Turner. I confess my idea is that it must have taken a long time. I do not propose to explain any recent formation of stationary radiants. I only learned from Mr. Whittaker that Mr. Denning claims a stationary radiant in the place of the Leonids, which came into the system only 2000 years ago. I did not contemplate anything that had been in the neighbourhood of the Earth less than (say) some millions of years; but I do not see why these stationary radiants should not be the result of the Earth's action for millions of years.

Col. Burton-Brown. I should like to ask Prof. Turner a question as to the diagram showing the curve of brightness of the corona. Am I correct in believing that it means that at about one-tenth of a diameter from the Moon's limb the intensity of light would be about half that of the Moon, and that at a distance of one solar diameter the light is about one-tenth of the intensity of the Moon?

Prof. Turner replied in the affirmative by reference to the diagram.

Col. Burton-Brown. The other question I should like to ask is if the photographic intensity is not practically greater than the visual intensity? that is, do not the observers who photograph the corona get a greater impression on their plates than observers who really see the corona? I do not follow from the visual line which is struck there (on the diagram) whether Prof. Turner means that to be the case or not.

Prof. Turner. I think the evidence is against that.

The President. We have two subjects before us, and we shall be confused if we do not keep discussion of them separate. I invite remarks on the latter paper—stationary points.

Mr. Whittaker. Prof. Turner has already referred to one or two of the difficulties which I raised on reading his paper, and another has occurred to me since I came into the room. I should like to say something about the difficulty which seems to me to arise in connection with this explanation of Mr. Denning's observations. Prof. Turner has proved beyond all doubt that if a meteor passes sufficiently close to the Earth the time at which it meets the Earth in subsequent encounters will be put forward. If, for instance, an Orionid would normally come on October 20th, it will come, as a result of this explanation, on October 19th in the next year, and on another return its orbit would be so changed that it would come on October 18th. This would account for a certain scattering of the meteors along the Earth's orbit, so it will account for the fact that meteoric displays extend over several months. But there is this difficulty in Prof. Turner's theory—it will only explain putting forward the time of the encounter, but not the putting backward. It will explain why the Orionid should come early in October, but not why it should come in November. We should have the display occurring earlier each year, and gradually increasing in brilliance until we come to the big display, and then suddenly dying out; but that does not accord with Mr. Denning's observations. Mr. Denning tells us, in the case of the Orionid swarm, the meteors begin to show in July or August, and go on until the following spring, having the maximum shower in October; and that seems to be the difficulty in the way of the acceptance of Prof. Turner's explanation. Another thing referred to by the President was that it would require millions of years to produce an effect on Prof. Turner's theory. Then Prof. Turner shows how the points can be displaced gradually along the orbit, but there is one point at which that process must stop, and that is the point where the meteor must, if it continued its path, strike the Sun. There is yet another critical point, namely, where the direction of the meteor is tangential to the orbit. These are the chief difficulties which at present stand in the way of the acceptance of the theory.

Prof. Turner. The case when the meteors struck the Sun would be comparatively rare, as the planes of the two orbits would have to coincide. As regards passing the apex of the orbit, I remember Prof. Alexander Herschel had also some difficulty; this, again, only happens when the plane of the meteoric orbit coincides with that of the Earth's orbit. As regards the Orionids, it seems to me possible that the radiant is really extended all round the Earth's orbit, but is apparently inactive for part of the year merely because the radiant is not visible, being too near the Sun.

The President. As Prof. Turner has said, this is a purely speculative paper, but it is extremely interesting, and such papers lead to discussions of the kind which we most value at our Meetings. As you all know, there is to be an eclipse of the Sun next month, and on that occasion the Astronomer Royal and Mr. Dyson are going to Portugal to observe it; and as we shall not have the pleasure of having Mr. Dyson with us at the next Meeting, I will ask him to give us some information as to the observations it is proposed to make.

Mr. Dyson. The Astronomer Royal is going to take photographs of the corona on a large scale, and he is also taking Mr. Davidson, of the Royal Observatory, to take photographs on a small scale, to obtain extensions of the corona. The work I am going to attempt is the photographing of the spectrum of the flash and of the corona, and is similar to that done by Capt. Hills in India, and the same spectroscopes will be used.

The President. I am sure we shall all wish Mr. Dyson every success in the observations he is going to undertake. The time of totality is extremely short, but no doubt they will economize it and make the best of it. We wish them all good fortune.

Mr. Lewis then read a paper on the orbit of the double-star β 883. This system was thought by Dr. See to have a period of 5½ years. Recent observations, unfortunately, did not support this period, but showed the period to more nearly approach that of Prof. Glasenapp, viz., 16 years. The difficulty arises from the fact that the components are of the same magnitude and very close, so that the observations may be turned through 180°. Indeed by a careful arrangement of the angles up to 1892, a small period can be obtained. (Diagrams were then thrown on the screen showing errors by equal areas, &c.)

Mr. Lewis then said the paper on observations of double stars in 1899 made at the Greenwich Observatory was hardly one for reading. They were made with the 28-inch equatorial. Altogether 563 pairs were observed during the year 1899, of which 130 were less than half a second of arc apart, 124 between half a second and a second, 126 between one second and two seconds, 80 between two seconds and three seconds, and 103 over three seconds; that last number looked rather large, but it was made up of stars like Aldebaran, β Cygni, and distant companions to the close pairs.

Mr. A. R. Hinks gave an account of his paper on "Observations of the Leonids made at the Cambridge Observatory on 1899 November 13, 14, 15." Preparations were made at Cambridge for observation of the meteors by several members of the University, and Mr. Hinks explained the method adopted for making charts of the stars, and showed the relative advantages for this purpose of different methods of projection.

The President regretted that the lateness of the hour did not allow remarks on this interesting paper.

The following papers were announced and partly read :—

H. H. Turner. "On the Stationary Radiants of Meteors: Reply to the Criticisms of M. Th. Brédikhine.

G. W. Hough. "Observations of Spots and Markings on the Planet Jupiter made at the Dearborn Observatory."

T. Lewis. "On the Orbit of β 883."

A. Fowler. "The Effects of Stellar Rotation and Revolution upon Spectrum-Lines."

A. Stanley Williams. "The Equatorial Current of Jupiter in 1898."

A. R. Hinks. "Observations of the Leonids made at the Cambridge Observatory on 1899 November 13, 14, 15."

Royal Observatory, Greenwich. "Results of Micrometer Measures of Double Stars made in the Year 1899 with the 28-inch Refractor."

Royal Observatory, Greenwich. "Measures of Double Stars from Photographs taken at the Royal Observatory, Greenwich."

W. E. Plummer. "Cometary Observations at the Liverpool Observatory."

H. C. Plummer. "An Application of Projective Geometry to Binary-Star Orbits."

The following gentleman was elected a Fellow of the Society :—

Thomas C. Bush, Somerville, Wells Road, Bath.

The following Candidates were proposed for election as Fellows of the Society :—

L. N. G. Filon, M.A., Fellow of and Lecturer on Astronomy at University College, London, Godwin House, St. Augustine's Avenue, South Croydon (proposed by H. F. Newall).

Forest Ray Moulton, Ph.D., Instructor in Astronomy at the University of Chicago, U.S.A. (proposed by G. E. Hale).

W. H. Pearsall, Higher Grade School, Dalton-in-Furness (proposed by H. A. Wassell).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE fifth ordinary Meeting of the tenth Session of the Association was held at Sion College on Wednesday, March 28, the President, *Mr. W. H. Maw*, in the Chair.

Six new members were elected, and the names of seven Candidates were passed for suspension.

The President announced with regret the resignation of one of their Secretaries, Mr. Schooling, owing to pressure of business engagements. The Meeting passed a hearty vote of thanks to Mr. Schooling for his services to the Association. The President then announced that the Council had elected Mr. Crommelin in Mr. Schooling's place.

Mr. Maunder announced the failure of the Eclipse Committee's negotiations for chartering the 'Tagus,' owing to that vessel and her sister-ship, the 'Nile,' both having been requisitioned by the Government as troopships. They had also failed to secure a smaller steamer, the 'St. Rognvald,' from lack of sufficient support. He thought, however, that that should not deter Members who had made up their minds to observe the eclipse from doing so. There were several routes open, although they could no longer go as one large party. The Royal Mail Co. had written to say that if a party for Oporto were made up for their boat leaving Southampton on May 11, they would call at that port. A return steamer would leave Lisbon on May 30. Ovar was only $1^h\ 40^m$ by rail from Oporto by the 8.15 train, and there were several trains back in the afternoon. Oporto might be made the headquarters, or it might be possible to hire a house at Ovar. Messrs. T. Cook & Sons were arranging a tour to Navalmoral, near Talavera. Another possible route was to Gibraltar by P. & O. steamer, and then by rail to Alcazar de San Juan, about 15 miles from the central line. There was also a line of steamers direct to Alicante. There was an overland tour arranged by Messrs. Cook & Sons to Algiers, and there were two lines of steamers running from Liverpool to Algiers. Mr. Maunder trusted that any Members who intended to go out would communicate with him in order that they might organize their observations as thoroughly as possible.

Mr. Crommelin gave particulars of an expedition that was being arranged by the French Astronomical Society. The price of the trip to Alicante from Marseilles and back was a little over £18. Of course Members would have to get to Marseilles, but it was hoped that return tickets at single fares would be procurable. The expedition left Marseilles on May 22, returning May 31.

The President said it was a source of great regret that the Association's own expedition had had to be abandoned, but they were all greatly indebted to Mr. Maunder for the immense amount of
he had undertaken in endeavouring to carry it through.
bers had some suggestions to make as to how the time

might best be employed between April 11 and the date of the eclipse, if any of them thought of adopting the Oporto route.

Mr. Chambers said that after consulting friends connected with Portugal, he had come to the conclusion that Lisbon was not a place worth going to, but that at Oporto there was a very good opportunity of most pleasantly and profitably employing the time between, say, the 15th and 28th May. Oporto itself was an exceedingly interesting city, and some 30 or 40 miles south there were two of the most beautiful specimens of Gothic architecture in Europe — Batalha and Alcobaça. Another point was that, although Ovar was very close to the Atlantic, fogs were not to be feared in the month of May. The fare from Southampton to Oporto and back by Royal Mail Co.'s boats was only £10 17*s.* 6*d.*, and the whole trip need not cost more than £20.

The Rev. S. J. Johnson contributed a paper on "Suggestions for the Observation of the Partial Phase of the Coming Eclipse." He advised observing:—(1) The times of first and last contacts; (2) Any irregularities in the dark limb of the Moon; (3) Especially if any of the black disk of the Moon could be seen outside the Sun; (4) The globular form of the Moon when superposed on the Sun; and (5) The earliest visibility of Venus with the naked eye.

Mr. Maunder said there was one observation which he trusted would be attempted in England during the partial eclipse here, namely, photographing the corona at the time of greatest obscuration. He thought it would be advisable to cover the entire disk of the Sun with a diaphragm placed as near the primary focus as possible. For this it would be desirable to have a photographic camera of considerable focal length, and to do it effectively there should be a guiding telescope rigidly attached to it.

Col. Burton-Brown alluded to probable difficulties with the customs authorities in Spain and Portugal, and hoped that the officials of the British Astronomical Association would communicate with the British Consuls at the different stations with the view of getting their instruments passed through without difficulty.

Mr. Crommelin read some notes upon "The Zodiacal Light and Gegenschein," from a paper by *Mr. F. J. Bayldon* (communicated to the New South Wales Branch of the Association). Mr. Bayldon said that in the clear air at sea the Light was seen to much greater advantage than from most places ashore. The cones of the Zodiacal Light were generally composed of milky white light, contrasting remarkably with the steely white of the Galaxy or the hard first signs of moonshine. The brightest portions of the Zodiacal Light frequently surpassed those of the Galaxy, and were seen before it in the evening and after it in the morning. A number of observations had also been obtained of zodiacal cones, evidently due to the Moon.

A report from *Capt. Molesworth*, Director of the Zodiac

Section, was read, in which he appealed for recruits for the section, and suggested observation both at sea and on shore during the coming eclipse expeditions.

Mr. Maunder was very glad to find the Zodiacal Light Section was setting to work. He was much interested in Mr. Bayldon's observations; he quite confirmed his description of the soft creamy tint of the Zodiacal Light as contrasted with the steely light of the Galaxy. Mr. Bayldon was not, of course, the first observer who had described a lunar Zodiacal Light, but the observations, so far, had been so few as to be open to question, and the subject was worth further investigation.

Mr. Goodacre read a paper on "The Duplication of the Canals of Mars," in which he supported Prof. Pickering's view that the capacity for seeing duplications distinctly was a personal one, which some observers possessed and others did not, instancing, as an illustration, the fact that Mr. Stanley Williams charted 84 narrow light streaks on the floor of the lunar crater Ptolemaus, whereas his own experience, and that of others of the members of the Lunar Section, pointed to the existence of only two well-defined streaks on the floor of that formation.

Mr. Maunder said he made a few experiments some years ago on the limit of sight, without optical aid, for small objects, and found that whereas the smallest dot that one could clearly see with the eye must subtend an angle of about 40 secs., he was perfectly conscious of a line when it only had a breadth of about 8 secs. At the same time it could not be called distinct vision—he was conscious of the line, but it was not sharply defined on the eye.

Mr. Chambers read some extracts from a Report of the Coloured Star Section, under the title of "A Catalogue of Red Stars for 1900"; and *Mr. Maunder* read portions of a report of the Meteoric Section by *Mr. W. F. Denning.*

A paper by *Mr. Gavin J. Burns*, on "The Proper Motion of Stars" (in which the writer said that, so far as the evidence derived from known proper motions was of value, it was entirely in accord with the hypothesis of a limited stellar universe), concluded the proceedings.

ROYAL METEOROLOGICAL SOCIETY.

On Tuesday, the 3rd instant, the Royal Meteorological Society celebrated its Jubilee, having been founded on April 3, 1850. A Commemoration Meeting was held in the afternoon at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. C. Theodore Williams* in the Chair. There was a large attendance, including delegates from a number of other scientific societies. In view of the Jubilee, the late Mr. G. J. Symons, F.R.S., was elected President at the Annual Meeting on January

17th, but owing to his being seized with paralysis he was obliged to resign the office. Mr. Symons had, however, prepared an Address giving a brief history of the Society, which Dr. Williams read, and then added some remarks of his own in appreciation of Mr. Symons and also on the work of the Society. Mr. Symons, after referring to early authors and observers, stated that the first English Meteorological Society was founded in 1823, but it soon languished and died. A second Society was formed in 1836, which likewise remained in existence only for a few years. The present Society was founded on April 3, 1850, by Mr. James Glaisher, F.R.S., with the co-operation of Dr. J. Lee, F.R.S., and others. This was called the "British Meteorological Society," but in 1866, when a Royal Charter was obtained, the name was changed to "The Meteorological Society." In 1882 Her Majesty the Queen accorded the Society permission to adopt the prefix "Royal." Both Mr. Symons and Dr. Williams referred in some detail to the work done by the Society, and the investigations which had been carried out under its auspices. The Society had no habitation of its own until 1872, when a room was engaged at 30 Great George Street; but owing to the increase of work and the growth of the library, additional accommodation had to be obtained on several occasions, the Society now having a convenient suite of rooms at 70 Victoria Street, Westminster. Dr. Williams, in concluding his address, stated that Mr. Symons had bequeathed to the Society £200, as well as the bulk of his large and valuable library.

Brief congratulatory addresses were delivered by Prof. J. J. Thomson, F.R.S., on behalf of the Royal Society; by Mr. E. B. Knobel, President, on behalf of the Royal Astronomical Society; by Mr. J. J. H. Teall, F.R.S., President, on behalf of the Geological Society; by Mr. R. C. Mossman, F.R.S.E., on behalf of the Scottish Meteorological Society; and by Mr. A. Wynter Blyth, M.R.C.S., on behalf of the Sanitary Institute. Prof. Dr. G. Hellmann presented an address from the German Meteorological Society, and also spoke in appreciation of the valuable work done by the Royal Meteorological Society.

In the evening a Conversazione was held at the Royal Institute of Painters in Water Colours, in Piccadilly, the guests being received by the President and Mrs. Theodore Williams. An exhibition of meteorological instruments, &c., was arranged in the banqueting-room, and Lantern Demonstrations were given (1) by Mr. T. C. Porter, of Eton, "showing the growth of eclipse of the shadow of the Peak of Teneriffe by the shadow of the Earth"; (2) by Col. H. M. Saunders, on "Clouds"; and (3) by Mr. W. Marriott, on Meteorological Phenomena and Portraits of Presidents of the Royal Meteorological Society. The Royal Artillery String Band played during the evening, and short concerts by the "Schartau" Part-Singers were given at intervals.

On Wednesday a large party of the Fellows were shown over the Royal Observatory, Greenwich, by the Astronomer Royal, after which they paid a visit to to the Painted Hall and Naval Museum.

In the evening a Dinner was held at the Westminster Palace Hotel, which was numerously attended, the President being in the Chair. The toast of the "Royal Meteorological Society" was proposed by Mr. W. N. Shaw, F.R.S. Gen. Sir H. W. Norman responded for the Army, Prof. Silvanus P. Thompson, F.R.S., for the "Delegates," and Dr. Pavy for the "Visitors."

Equatorial Spots on Jupiter.

SINCE December 10 last I have obtained a number of observations of Jupiter in the morning hours. The instrument employed was a 10-inch reflector, power 312. The weather has, however, been unfavourable generally for telescopic work, and definition has been rarely very good.

A considerable number of markings which were seen in 1898 and 1899 have been identified and are still visible. These include a series of light and dark spots near the equator, and many similar objects ranging along the N. side of the great N. equatorial belt. The various markings on Jupiter apparently remain in existence for much longer periods than is commonly supposed, and if closely followed their identification from one opposition to another can be effected without difficulty. Many of the features, and especially those near to the equator, undergo marked changes in appearance and sometimes also in motion, so that it becomes necessary to be very careful if the process of identification is to be perfectly satisfactory.

The equatorial spots are extremely numerous. They are distributed along the interior sides of the two large equatorial belts, and large irregular markings are sometimes noticed on the equator. The spots on the N. margin on the S. equatorial belt have been pretty generally observed during the last 20 years, and one of the most curious features in connection with them (and, indeed, with several other spot-zones of the planet) is the differences which they individually exhibit. In 1898 I found that the spots varied one with another in their rotation-periods to the extent of about 16 seconds. Similar differences are observable in the objects now visible, and I have selected two pairs as representatives of a numerous class. During the past two years the first pair of markings, ① and ②, have shown a mean rotation of $9^h\ 50^m\ 27^s$, while the other pair, ③ and ④, have given a period of $9^h\ 50^m\ 20^s$, or 7^s less. As it is desirable to identify the various markings from year to year, I give the observations made here in detail, and have added an ephemeris which may serve as a guide to the places of the same objects in the few ensuing years. It is of course impossible to be absolutely certain as to the identification of markings of this character, as there occurs an unusual break of several months in the observations when Jupiter is near the Sun, but there seems little doubt that the observations relate to the same features. I obtained very few views of this planet in 1896 and 1897, but it is highly probable that the objects alluded to have been visible for a long period.

	Observed Transit Times and Longitudes.					Computed Longitudes.	
	①. White Spot.		②. Dark Spot.			W.	D.
	T. T.	λ.	T. T.	λ.		λ.	λ.
1898.	h m	°	h m	°		°	°
April 1...	...	...	8 36	155·8			
7...	...	...	12 14	156·8			
8...	...	...	7 51	154·3			
12...	10 4	147·4	10 17	155·3			
17...	8 8	146·6	8 22	155·2	April 15...	145	155
19...	9 15	143·4	9 37	156·8			
May 12...	8 19	141·4	8 34	150·5			
14...	9 35	143·5	9 53	154·5	May 15...	143	153
28...	...	...	8 35	156·8			
June 22...	8 33	140·0	8 53	152·2	June 15...	141	151
July 15...	...	...	8 8	152·5	July 15...	138	148
17...	8 58	138·5	...	...			
1899.							
June 8...	9 7	117·7	9 23	127·5			
13...	7 13	117·6	7 28	126·8			
15...	8 22	115·5	8 33	122·2	June 15...	115	125
29...	...	...	7 17	125·5			
July 8...	...	...	7 44	122·2	July 15...	113	123
31...	6 44	114·1	7 2	125·1			
Aug. 9...	...	...	7 30	121·6			
11...	8 32	114·8	8 49	125·1	Aug. 15...	111	121
16...	6 35	112·0	6 55	124·2			
18...	7 50	113·1	8 10	125·3			
23...	5 47	106·5	6 15	123·6			
1900.							
Feb. 7...	19 8	97·1	19 38	109·3	Feb. 15...	97	107
19...	16 47	99·4	...	...			
21...	17 53	95·5	18 14	108·3			
Mar. 16...	16 49	88·5	17 20	107·4	Mar. 15...	95	105
25...	17 25	91·9	17 47	105·4			

Spots lose 2°·2 ($3^m\ 38^s$) per month relatively to Mr. Crommelin's System I. ($9^h\ 50^m\ 30^s$). Rotation Period $=9^h\ 50^m\ 27^s$.

	Observed Transit Times and Longitudes.					Computed Longitudes.	
	⑧. Dark Spot.		④. White Spot.			D.	W.
	T. T.	λ.	T. T.	λ.		λ.	λ.
1898.	h m	°	h m	°		°	°
Mar. 22...	10 15	75·9	...	...	Mar. 15...	77	87
April 7...	10 5	78·1	10 14	83·6	April 15...	69	79
16...	10 23	70·9	10 37	79·4			
23...	...	...	9 55	79·6			
30...	8 48	64·3	9 2	72·8			
May 14...	...	...	7 50	79·5	May 15...	62	72
16...	8 42	66·9	8 55	74·8			
June 10...	8 50	57·2	9 22	76·7	June 15...	54	64
17...	...	...	8 27	67·7			
19...	9 13	51·3	...	...			
July 3...	7 53	50·8	8 4	57·6			
5...	9 2	48·5	9 18	58·3			
10...	...	...	7 33	62·5	July 15...	47	57
1899.							
May 6...	9 40	325·6	...	...	May 15...	330	340
June 14...	8 22	317·6	...	...	June 15...	323	333
16...	9 39	320·2	9 54	329·4			
21...	7 50	323·1	8 3	331·0			
23...	8 48	314·1	9 10	327·5	Aug. 15...	308	318
Aug. 19...	8 43	303·1	...	...			
24...	6 55	305·7	7 16	318·5			
26...	8 10	306·8	...	...			
Sept. 2...	7 23	301·9	...	...			
9...	6 49	304·9	...	...			
14...	4 50	300·8	5 13	314·8	Sept. 15...	300	310
Dec. 13...	19 44	277·0	20 1	287·4	Dec. 15...	277	287
29...	19 45	281·4	...	...			
1900.							
Feb. 6...	...	...	18 30	270·0	Feb. 15...	262	272
20...	...	...	17 17	275·6			
Mar. 13...	...	...	15 12	275·5			
15...	15 57	258·8	16 20	272·8	Mar. 15...	255	265
17...	17 0	253·1	17 24	267·7			

Spots lose 7°·6 (=12^h 28^m) per month relatively to Mr. Crommelin's System I. (9^h 50^m 30^s). Rotation Period $=9^h$ 50^m 20^s.

Ephemeris of Four Equatorial Spots *.

	① White.	②. Dark.	③. Dark.	④. White.
	λ.	λ.	λ.	λ.
1900.	°	°	°	°
April 15....	92	102	247	257
May 15....	90	100	239	249
June 15....	87	97	232	242
July 15....	85	95	224	234
Aug. 15....	83	93	217	227
1901.				
Jan. 15....	72	82	179	189
Mar. 15....	68	78	154	164
May 15....	63	73	149	159
July 15....	59	69	133	143
1902.				
Mar. 15....	41	51	72	82
July 15....	32	42	42	52
1903.				
May 15....	11	21	326	336

Thus if the markings remain visible and the relative motions of the objects are maintained until 1902 July 15, spot ③ will overtake ②. The former completes a revolution of Jupiter relatively to the latter in about 5 years and 7 months. It is hoped that observers will keep these interesting objects under notice in future years.

In another letter I hope to make reference to several other forms often observed on Jupiter during the last two years, and affording interesting data as to the rate of motion of the different currents.

W. F. Denning.

Bristol, 1900, Mar. 19.

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* I have adopted 10° as the distance separating ② from ① and ④ from ③; but the interval between the markings seems greater now than formerly.

Note on Colonel Tupman's Paper on Tycho Brahe's Meridian Observations of the Sun.

ALTHOUGH the reduction of observations made three hundred years ago can only in rare cases be of value for correcting our modern tables, it is always instructive to see what degree of accuracy was attainable at that time, and Colonel Tupman's examination of some of Tycho Brahe's observations is therefore of considerable interest.

It is a great pity that so few of these observations have been properly published. The observations of comets were printed more or less completely by Pingré and Schumacher, and all of them were published at Copenhagen in 1867, while the observations of planets made in the year 1593 were printed in the 'Mémoires de l'Académie' for 1757 and 1763, but otherwise the only generally accessible source is the 'Historia Cœlestis' of Albert Curtz, which Colonel Tupman has used. Though he remarks that "there are a great number of misprints in the volume," Colonel Tupman does not appear to be aware how utterly worthless this compilation is. I shall, however, not enter into any further details on this subject, as I have given a very full account of the origin and character of the 'Historia Cœlestis' in my book on Tycho Brahe, pp. 372–373. Curtz apparently did not know that the incomplete set of volumes at his disposal were not the original observations, but only unfinished and (worst of all) uncollated copies of these, most carelessly written and teeming with errors. The original observations are still at Copenhagen, and a collated copy, prepared for publication about 1669 by Erasmus Bartholin, the discoverer of double refraction, is now at the Paris Observatory [*]. It is, however, not likely that Colonel Tupman's principal conclusions can be materially affected by the clerical errors of Curtz's copy, though the general accuracy of the results would no doubt have been greater had the original observations been accessible.

With regard to the latitude, Colonel Tupman states that Tycho frequently used the value 55° 54′ 15″, which is also that given in Halley's tables. This value was found by Picard in 1671, but Tycho generally adopted 55° 54′ 30″, though he was not particular about a few seconds. Though there are very few traces left of his buildings, the remnants of the ramparts and the still existing well enable us to fix the sites of Tycho's various instruments within a few feet. The latitude of Uraniburg according to modern determinations is 55° 54′ 26″; the longitude east of Paris is $0^h\ 41^m\ 26^s$. See Conn. des Temps, 1836, page (117).

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[*] This must be the copy referred to as B 4, 15–20, in the list of MSS. in the 'Annales de l'Obs. de Paris,' t. xxi. p. F 24. A few lines above, M. Bigourdan states that Bartholin's copy was returned to Denmark more than 150 years ago. But it was quite certainly *not* returned, and Jeaurat states that Lacaille had it bound, and that De l'Isle made another copy of it (Mém. de l'Acad. 1763, p. 87). I believe the latter copy is in French and is not complete. Probably the folded "cahier in f°" mentioned by M. Bigourdan as being inscribed "Manuscrit Erasme Bartholin" should by right belong to one of the six folio volumes, and has only by accident been put into the portfolio containing Pingré's MS.

Colonel Tupman remarks that a correction for zero to the observed altitudes may have been applied mentally. There is, however, abundant evidence in the original observations that the instrumental errors were recorded from time to time, and that whenever none are mentioned the instruments were kept so well adjusted that a correction was not required.

The curious systematic errors in the solar observations found by Colonel Tupman are on the very limit of accuracy attained by Tycho, and the suggestion (on p. 133) that the surface on which the image of the Sun was received would in summer and winter be presented to the observer in a very different manner, probably gives the clue to the very minute systematic errors found. Colonel Tupman has examined a number of observations of stars made in 1582 in order to see if there might be a considerable error in the zero of azimuth. As may be seen from p. 388 of my book, I examined this question more than ten years ago, using the very same materials, and coming of course to the same result, viz., that the error of azimuth of the quadrants was certainly less than a minute of arc.

Though it has nothing to do with Tycho Brahe, perhaps I may take this opportunity to answer a question asked by Colonel Tupman in a footnote. A list of thirty-four known copies of the second volume of the 'Machina Cœlestis' of Hevelius is given by Delambre in his 'Histoire de l'Astronomie moderne,' t. ii. p. 467, but as many of the 34 copies belonged to private individuals, it is hard to say where they are now. The list cannot be complete, at least I have seen one copy, not mentioned in it, in the Royal Library of Copenhagen, with an autograph dedication on the title-page to King Christian V. of Denmark. J. L. E. Dreyer.

The Unknown in Astronomical Spectroscopy.

One of the most striking features of recent astronomical spectroscopy is the gradual disappearance of the "unknown lines" which there had been a tendency to regard as indicative of the presence of non-terrestrial matter in some of the heavenly bodies. Progress in this direction seems likely to have an important bearing on our ideas of cosmical evolution, and it is interesting to recall the various steps which have so extended our resources in the interpretation of the spectra met with in a survey of the heavens.

The identification of lines which long appeared inexplicable from the point of view of our terrestrial chemistry has been made possible by work along five principal routes:—

(1) The search for elements not previously known to exist upon the Earth.

(2) The more exact determinations of the wave-lengths of lines, both terrestrial and celestial.
(3) The discovery of additional spectra of well-known substances.
(4) The application of the laws of spectrum series.
(5) The investigation of enhanced lines.

Thus it was not until helium was discovered by Prof. Ramsay that the important lines of that gas occurring in the spectra of stars and nebulæ were removed from the category of unknown lines.

Again, the better comparison of wave-lengths resulting from the use of improved instruments in the laboratory and observatory has led to the certain recognition, by McClean, Lockyer, and Gill, of lines of oxygen, nitrogen, and carbon among the previously unknown lines in the spectra of many of the stars of the Orion type.

Continued researches on the multiple spectra of the elements are also full of promise. An admirable illustration is afforded by Lockyer's discovery that silicium has no less than four spectra, each of which is represented in stellar spectra belonging to different groups. One set of lines, produced in the electric arc, appears among the Fraunhofer lines of the solar spectrum; another set, brought out by the electric spark, is found in Sirius, α Cygni, and other stars of intermediate temperature; while two other sets, which appear in vacuum-tubes, occur in stars of successively higher temperatures, represented by γ Orionis and ζ Orionis respectively. The group of silicium lines in γ Orionis was also traced independently by Mr. Lunt at the Cape Observatory.

Another very remarkable case of identification is that of a series of lines which occurs in ζ Puppis and other stars. Prof. Pickering found that these lines occupy positions in the spectrum which can be calculated from the formula for the well-known series of hydrogen lines, and although experiments to reproduce this spectrum artificially have so far failed, we seem justified in concluding that the lines in question are due to hydrogen in a particular state.

Perhaps of a more comprehensive character is Sir Norman Lockyer's interpretation of a great number of lines in the spectra of the hotter stars as being simply the lines of familiar elements produced at high temperatures. As a result of the study of these "enhanced lines," we now know as much about the spectra of such stars as α Cygni and Sirius as we do about the solar spectrum itself, and the same investigation accounts for a large proportion of the lines in the spectrum of the chromosphere as photographed during eclipses of the Sun. It has not yet been possible to isolate the enhanced lines in the laboratory, but the fact that their brightness increases with the temperature of experiment is a sufficient indication of their character. In some stars these lines appear without the lines produced at lower temperatures, while in others the two groups of lines occur together.

In spite of the fruitful researches of recent years, however, we are still in ignorance of the chemical significance of several lines of great importance. Chief among these are lines which occur in the spectra of nebulæ (more particularly those at 5007·05 and 4959·02), those in the spectrum of the solar corona (among the brightest being 5303·7, 4231·3, and 3987·0), and certain lines which appear in the spectra of the Wolf-Rayet stars (among them being 5813, 5693, 4652, and 4633).

It seems by no means too much to expect, however, that work along the various lines indicated will ultimately result in the complete elimination of the so-called "unknown elements" from celestial chemistry. A. FOWLER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

A very Ancient Eclipse.

GENTLEMEN,—

Prof. Stockwell has asked me to communicate the following to you in case you should think it of sufficient interest to print in the *Observatory*. Some years ago it occurred to him that the "horror of great darkness" which fell upon Abraham "when the Sun was going down" (Gen. xv. 12) might be caused by a total or nearly total eclipse, and having kept the subject before him, he now writes:—

"I have since spent some time in search of such an eclipse, and have quite recently found one that very completely tallies with the description. I find that on July 13, B.C. 1927, a great eclipse passed almost centrally over Jerusalem at about three o'clock in the afternoon, so that it took place 'when the Sun was going down.' It was not quite total, however, but its magnitude was 11·87 digits, being an annular eclipse."

I do not know whether Prof. Stockwell, in writing to me, wishes me to express any opinion on the point. But I cannot help remarking that whilst a record of an eclipse (and of course we are by no means sure in this case that one is alluded to) may be very useful in determining a date in chronology when the uncertainty about it is limited to a few years, the difficulty in applying it greatly increases when this is considerable. We all know that modern Egyptologists have come to the conclusion that the Exodus took place about two hundred years later than was formerly supposed, the date of the accession of Meneptah II. (in whose reign it took place) being about B.C. 1300. The Septuagint translation of the history gives a very different duration to the sojourning in Egypt from that of our copies of the Hebrew. With Prof. Sayce, it seems to me that the latter is to be preferred, which makes it four hundred and thirty years, so that the going down into Egypt would have been about B.C. 1725. The date, then, of the above event in the life of Abram or Abraham may have been about that of the eclipse calculated by Prof. Stockwell, but we cannot claim more than a measure of probability for it, especially under our uncertainty of the cause of the patriarch's "horror." Prof. Stockwell does not say for how many years he has examined the eclipses which passed over Canaan before and after the above.

Yours faithfully,

Blackheath, 1900, Apr. 10. W. T. LYNN.

Periodically Recurrent Disturbances in the North Temperate Belt of Jupiter.

GENTLEMEN,—

When a theory or suggestion only awaits the lapse of a comparatively short period to be either verified or negatived, it seems hardly politic to enter into a controversy as to whether the inception of the theory or the period of the phenomena concerned is justified by the evidence. The idea of recurrent north-temperate markings on Jupiter, which I ventilated in *Monthly Notices*, vol. lix. p. 76, is not, however, objected to in principle by Mr. A. Stanley Williams; he only finds fault with the "period of about 10 years" which I deduced as the most probable. I should have let his remarks pass unnoticed (thinking as I do that the matter may well be left for observations in the immediate future to determine), but he has so imperfectly, if not incorrectly, referred to the facts that a reply seems necessary.

Mr. Williams thinks that the oblique belt of 1860 had no significance in connection with the outbreaks of spots in later years. He utterly ignores the observations of Birmingham, Cook, Gledhill, Whitley, and others in 1870, and also those of Barnard at the close of the opposition of 1890, and then jumps to the conclusion that we are left with "only two real indisputable facts to go upon with regard to periodicity," namely the outbreaks of 1880 and 1891, and that a period of about 12 years will satisfy the case. This is, however, quite inconsistent with the evidence. Remarkable disturbances, vouched for by trusted observers, affected this region of Jupiter as follows:—

Date.	Observers.
1860 March	Baxendell, Jacob, Long.
1870 Aug.–Sept.	Birmingham, Cook, Gledhill, Whitley.
1880 October	Dennett and many others.
1890 Oct.–Nov.	Barnard, and others in 1891.

These probably associated phenomena indicate an approximate period of about 10 years, and we have no right to throw over some of the best observations in the belief that the chief recurrences on

Jupiter must necessarily be commensurate with the time of his revolution round the Sun. The cycle of the Sun's spots is by no means perfectly regular in its operation, and the character of Jupiter's variations may show similar vagaries, but the records we possess are not sufficient to enable us to pronounce definite judgment on the matter.

Mr. Williams says that "the north temperate belt is seldom or never entirely free from spots." No doubt there are slight irregularities and condensations frequently evident in the belt, but they are obviously different to the conspicuous bluish-black spots which have formed special outbursts at the end of the last few decades. I can only say that my 10-inch reflector has often shown me the belt free of irregularities, but I lay no claim to the faculty possessed by some people of seeing spots on any object at any time and in any sort of weather.

While writing on this point I would take the opportunity of referring to another in connection with this planet. Mr. Williams observed a number of transits of the great red spot during the last opposition, and gives the times in *Ast. Nach.* 3596. Now it is apparent from these that he saw the middle of the red spot *preceding* the hollow by about 3½ minutes. On the other hand, Messrs. Antoniadi, Flammarion, Gledhill, Phillips, and myself saw the spot *following* the hollow about 3 minutes. Mr. Williams explains this remarkable discordance on the assumption that other observers saw only the incomplete or denser part of the spot, while his observations refer to the entire elliptical outline of the object. But this is quite a misconception on Mr. Williams's part, though if admitted it would readily remove the difficulty. I perceived the outlines of the spot with very fair distinctness on several occasions, and the middle of it slightly followed the centre of the hollow; this was also the experience of other observers. Under the circumstances Mr. Williams is by no means justified in putting himself in the right and others in the wrong. He says "the spot itself was by no means difficult," though "poor seeing nearly always prevailed." Other observers found it impracticable to secure transits of the spot* under indifferent definition, and therefore contented themselves with noting the position of the hollow. I for one would most willingly accord special merit wherever it may be unquestionably due, but I quite fail to see from the evidence before us that we are called upon to concede phenomenal powers to any single observer at the present time.

I am, Gentlemen,

Your obedient Servant,

[illegible] W. F. Denning.

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* The Rev. T. E. R. Phillips [illegible] exceedingly faint and [illegible]

Proposed Alteration in the Russian Calendar.

Gentlemen,—

May I say a word on this subject, as I have seen a more complete account of the proposed alteration than had appeared when your last number was issued? It was in the 'Companion to the British Almanac' for 1882 that I first pointed out that the calendar would be kept almost exactly right by the regular omission of a leap-year each 128th year. This suggestion I repeated in the number of *Nature* referred to by you (that for June 11, 1896, vol. liv. p. 126), my attention having been called to the subject by a proposal of a somewhat different kind for modifying the Gregorian Calendar, which had just been brought forward by M. Auric in the *Comptes Rendus* of the French Academy. My idea was to start from this year, accepting the omission of a bissextile day last February, and then enact that the next omission should be made in A.D. 2028. That would then be the first departure from the existing scheme, as A.D. 2000 will be a leap-year according to either Julian or Gregorian reckoning.

But the present Russian proposal appears to be to carry out this omission of a leap-year every 128th year more thoroughly, by taking it as if this had been done from the Christian era, and decreeing that every year divisible by 128 without remainder should not be a leap-year. As there have now been fourteen such years since A.D. 0, the proposal would imply the abolition of fourteen days, and counting the Russian days fourteen ahead—to-day, for instance, which is with them March 22, being called April 6, two days ahead of ours. Then in A.D. 1920 they would omit another leap-year and their count of days would differ by only one day from ours. Whether the rest of the Christian world would agree to fall in with this and abolish two days whilst the Russians abolished fourteen (making our count of days two later), it is not for me to say. Nor does one quite see what is gained by making chronological dates in the past as if they had been what they were not when it would suffice to start from the present epoch. However, in this country at any rate we are used to compromises, international and otherwise.

Yours faithfully,

W. T. Lynn.

Blackheath, 1900, April 4.

Duration of Eclipses.

Gentlemen,—

As an appendix to the interesting letter of Rev. S. J. Johnson in the last number, it may be of interest to point out that the eclipse of 1912 will be total for a few seconds (probably not more than 7 secs.) in Portugal and Spain. The central line runs approximately from Aveiro to a point some 10 miles east of Oviedo. It then crosses France, passing 2 or 3 miles south of Paris. On the west coast of France it will probably be just total, and near Paris it will be annular for 2 or 3 seconds. Oppolzer's data make it total for 7 seconds here, but, as was pointed out, his durations of totality are systematically too great.

The notes on the durations of certain eclipses given in the last number may be supplemented by the following:—

Year.	Duration of Totality.		Year.	Duration of Totality.	
	m	s		m	s
132 A.D.	7	27	1868 A.D.	7	10
150 "	7	24	1886 "	6	58
1814 "	6	47	1904 "	6	42
1832 "	7	2	1922 "	6	23
1850 "	7	6			

These are from Oppolzer's data and are all probably at least 14 seconds too long. A. C. D. CROMMELIN.

PUBLICATIONS.

HARVARD ANNALS, VOL. XXXII.—Not the least interesting of the many volumes issued from Harvard College Observatory are those which have "Miscellaneous Researches" for their sub-title, and in which are collected various investigations made at the Observatory, each of which is of itself not large enough to fill a book. Such a volume we have now before us, consisting largely of tables of reduction of observations of variable stars, which scarcely lend themselves to epitomizing, but whose scope may be briefly stated. There is a series of observations of variable stars made by Argelander in the years 1869–1871, here published for the first time, with a previous series made by him in the years 1838 to 1867, all reduced by help of the Harvard photometric magnitudes of the comparison stars used by Argelander. There are also reductions of similar series of variables made by Schönfeld and by Schmidt. Other photometric work in the volume consists of determination of the brightness of the asteroids and of the satellites of Mars, of nebulæ, and some comets, seven in all. There is a long series of the Comet 1881 III. Mr. Arthur Searle is responsible for three of the memoirs in the volume; the first of these, on the Gegenschein, gives the position of this object, if so it may be called, as determined by various observers in the years 1891 to 1894. This is an elusive phenomenon, and neither the facts concerning it nor the theory can as yet be considered established, but Mr. Searle says that as far as the work has gone at the present time it appears probable that the Gegenschein exhibits no noticeable parallax, and that it does not deviate perceptibly from the ecliptic, its course lying probably in a great circle, the ascending node of which is near the longitude 90°, whilst its inclination is 2° or possibly more.

Mr. Searle's second memoir is on a cognate subject, and gives the position of the luminous bands visible in and near the zodiac which have been seen by several observers. The contour-lines of these luminous bands are given in diagrams, and the chief result of the observations was "to indicate the presence, as permanent and invariable features of the sky, of two bands or faint branches of the Milky Way, which might readily be mistaken for zodiacal light in parts of their courses." The third memoir to which we have referred is on an altogether different subject to all the others in the book, as it gives the results of meridian observations of position of certain stars. Mr. Searle discusses here various small points of the nature of error of observation. For one thing, he says that the lighting, if effected by means of an imperfect elliptical reflector, may not be exactly symmetrical over the whole field and the star may appear to be on a wire, before or after it has actually reached it. In other words, the faulty lighting causes a displacement of the wires. The amount of displacement, it is suggested, depends on the focussing of the eyepiece, so that it differs for observers with different eyesight. These points, as Mr. Searle says, have long been realized by astronomers, in fact the subject was investigated by R. Wolf, of Zurich, in 1869, but he gives his experience and that of his colleague, Mr. Dunne, which forms a valuable contribution to the subject, although no quantitative result is arrived at. This point has a bearing on the question of time-determination with a reversible transit, the results of which frequently differ in the two positions of the instrument; it may be worth while pointing out that if the lighting did apparently thus alter the positions of the wires, the collimation determined from polar stars by reversal should agree with that from equatorial stars. But it does not always do so, which militates against the acceptance of this as the whole cause. Mr. Searle further discusses the change of personal equation with magnitude of the star observed, also a change of habit during an evening's work, which sometimes appears and is often ascribed to change of clock-rate. Altogether this is a most interesting memoir to those who work at fundamental practical astronomy, and will repay reading.

Recent and Coming Eclipses [*]. — As we noticed the first edition of this book no later than November 1897, there would be little need now for anything beyond a brief mention of the appearance of a second edition, if that second edition were, as is often the case, a mere revision of the first. But Sir Norman Lockyer has added two very important sections since its first appearance—the one occupying no fewer than six chapters, and dealing with the eclipse of January 22, 1898; the other referring to the assistance rendered to him by officers and men of the Royal Navy. It is true that in the first edition Sir Norman described the spirit and energy with which he was supported by Captain King Hall and the officers and men of H.M.S. 'Volage' in 1896. But the weather was unpropitious on that occasion, and however well the naval assistants seemed to shape, it was open to cavillers to object that they would not stand the searching test of actual work under a clear sky. The remarkable efficiency shown at Viziadrug by the volunteers from H.M.S. 'Melpomene' has rendered such a cavil impossible, and "Jack" has proved himself "the handy man," not merely on his own element, nor even on land, but also in an attack on the heavens. It is interesting to note that Captain Chisholm Batten told Sir Norman when he left that there had not been a single punishment on his ship for six weeks, so great was the interest taken by the crew in the work of eclipse preparation. The way in which Sir Norman and Mr. Fowler organized and drilled this volunteer force is well told, and here and throughout the book we realize how Sir Norman's great experience and keen grasp of eclipse problems have won for him the striking successes he has attained in this field. He has left nothing to chance that it was in his power to provide for; and though chance has done him an ill turn by giving him clouds instead of corona in 1886 and 1896, the record of progress made from 1871 to 1898 is one in which he shows a pride, most justifiable, and with which his readers will sympathize.

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[*] 'Recent and Coming Eclipses.' By Sir Norman Lockyer, K.C.B., F.R.S. Second Edition. Containing an Account of the Observations made at Viziadrug, in India, in 1898, and of the Conditions of the Eclipses visible in 1900, 1901, and 1905. London: Macmillan & Co., Limited, 1900. Price 6s.

The book deals practically with Sir Norman Lockyer's work alone; other workers in the same department are either not mentioned at all, or referred to in the most incidental way. Thus we noticed in our former review the entire neglect to mention Mr. Stone in the account of the Novaya Zemlya expedition of 1896, and we are sorry to remark that that neglect has not been repaired, though the regretted death of that distinguished astronomer would have made such reparation most seemly. Again, the only photograph of the 1898 eclipse which Sir Norman Lockyer reproduces was taken at the British Astronomical Association's camp at Talni, a camp which is somewhat pointedly excluded from the list of stations on p. 144, and its members are ignored. But taking the book as intended to deal only with Sir Norman's own achievements—and it must be remembered that no living astronomer has an eclipse experience at all rivalling his—it forms a record of exceptional interest and importance at the present time, when the eclipses of the current month and of May 1901 are occupying so many minds.

MEASURES OF DOUBLE STARS *.—We are always glad to welcome Prof. Glasenapp's measures of double stars, ever remembering that to him we owe measures of southern stars at a time when but few observers were giving them attention. The present forms the fifth series made at St. Petersburg and at Domkino in the Crimea, and were made during the years 1897 and 1898, so

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* 'Méaures micrométriques d'étoiles doubles faites à Domkino et à St. Pétersbourg.' Par le Prof. S. de Glasenapp. St. Pétersbourg, 1899.

that it completes the record from 1892–1898. The measures now before us are of northern pairs, but of that class liable to neglect. They are not of sufficient difficulty to attract the notice of observers with the large refractors, and possessors of small instruments have lately appeared to imagine that as the large refractors were engaged in double-star work there is no room for them. This, as we take every occasion to point out, is wrong; there is more for them to do than ever before. Prof. Glasenapp gives us measures of 300 pairs, of which about one-fourth are less than 5″ apart.

NOTES.

Comet Notes.—The following elements and ephemeris (for Greenwich midnight) of Giacobini's Comet (*a* 1900) are by Mr. Perrine:—

T ...... 1900 April 29·0781, G.M.T.
ω ...... 24° 36′ 57″
☊ 40 24 39
i 146 25 22
log q .... 0·12348

	R.A. h m s	N. Dec. ° ′
May 1....	1 31 0	18 33
5....	1 28 46	19 38
9....	1 26 18	20 46
13....	1 23 36	21 56
May 17....	1 20 34	23 9
21....	1 17 9	24 25
25....	1 13 16	25 46
29....	1 8 47	27 11

The brightness throughout May is about the same as at discovery. The comet will scarcely be seen till nearly the end of May, being too near the Sun.

Bulletin Astronomique for March contains an article by M. G. Fayet on an original orbit of Comet 1892 II.

It will be remembered that Herr Thraen announced a few years ago that the hyperbolic orbit of Comet 1886 II. was due to planetary action, and that the original orbit was elliptic. M. Strömgren, on the other hand, concludes that the original orbit of Comet 1890 II. was hyperbolic. He also points out a flaw in Herr Thraen's researches on Comet 1886 II., viz., that the latter had measured its position from the Sun and not from the centre of gravity of the solar system. When a comet is near the Sun and moving swiftly, this is of little consequence, but when a comet is at a great distance and moving slowly it becomes important, for the speed of the Sun round this centre of gravity then becomes comparable with the comet's speed. Thus the question of the original form of the orbit of Comet 1886 II. should be studied anew, Thraen's result being untrustworthy.

M. Fayet has made an investigation on Strömgren's lines on the original orbit of Comet 1892 II. (Denning), which was moving, according to M. Steiner, in the following hyperbolic orbit when near the Sun:—

T	1892 May 11·26194 Berlin M.T.	
π	22° 44′ 3″	1890·0.
☊	253 24 10	
i	89 41 54	
log q	0·29462	
e	1·00035	

He finds that the original orbit was elliptical, with $e = 0·9984$.

A. C. D. C.

Minor Planet Notes.—Four new planets have been discovered: FC by Schwassmann at Heidelberg on March 28, FD by Keeler at Lick on December 6 last. The latter planet is only of the 17th magnitude, and publication of the discovery was delayed. FE, FF were discovered by S. Hirayama in Japan on March 6.

FA proves to be one of the nearer members of the group, having a mean daily motion of 1098″. The identity of FB with (117) Lomia is confirmed.

The following planets have received permanent numbers (*Ast. Nach.* 3635):—

Planet.	Discoverer.	Date.	Number.
EX	Coddington.	1899 Oct. 4	445
ER	Wolf—Schwassmann.	Oct. 27	446
ES	" "	Oct. 27	447
ET	" "	Oct. 27	448
EU	" "	Oct. 31	449
EV	" "	Oct. 31	450
EY	Charlois.	Dec. 4	451

EN, EP, EQ, EW, EZ proved to be identical with (85) Io, (32) Pomona, (161) Athor, (110) Lydia, (415) respectively. EO does not receive a permanent number, not having been sufficiently observed.

The planets (440), (445) have been named Theodora and Edna respectively.

Bulletin Astronomique for March contains some computations on the subject of the gaps in the zone of minor planets by M. O. Backlund. He shows that the mean motions 587″·6 and 607″·0 are critical values, between which the motion of a planet would appear to be unstable, but his researches on this point are not yet concluded.

The approaching opposition of Eros will afford a very favourable opportunity for improving our knowledge of the Sun's parallax. *Ast. Journ.* No. 480 contains an article by Prof. Newcomb, giving diagrams of the hemisphere of the Earth turned towards the planet at different times, and suggesting suitable combinations of stations for photographic observations. It is hoped that a very large number of observers will co-operate. Owing to its high north declination, the planet will be out of reach of the observatories of the southern hemisphere till January next. A. C. D. C.

THE ECLIPSE OF MAY 28.—The Portuguese Government are offering most generous aid to astronomers who will visit the country to observe the solar eclipse. It is necessary for a visitor to provide himself with a certificate emanating from a recognized observatory, and authenticated by the consular agent, which should bear his name and the number and description of the packages he has with him, when the following favours will be granted:—

The custom-house authorities will give facilities for the admittance of their luggage, and the cases containing instruments will be admitted free of charge.

Guards of police, or of soldiers if necessary, will be provided to secure privacy for the observing camps, and in cases where observing-stations are chosen far from centres of habitation, the army will provide tents.

The telegraph authorities will set up temporary stations if necessary, and arrangements will be made to supply the accurate time of the Lisbon Observatory.

The Lisbon Geographical Society offer abundant help, and put their rooms at the disposal of astronomers.

The railway companies have consented to make a reduction to half fares. The State Railways will even supply gratuitous tickets.

To obtain these concessions astronomers should communicate with Capt. Campos Rodrigues, Director of the Lisbon (Tapada) Observatory, who has kindly issued a circular giving the above information.

A METHOD OF PRINTING RÉSEAUX. — As it is sometimes necessary to be able to imprint a réseau on a photographic plate already developed, the following recipe for doing so, by M. Henri Bourget, of the Toulouse Observatory, published in the *Bulletin Astronomique* for March, is worth recording:—

Make a 1 or 2 per cent. solution of bichromate of ammonia, and a saturated solution of carmine in this.

Immerse the plate in the solution for three or four minutes, taking care to get rid of all air-bubbles. Put it to dry in the dark room, in a place free from dust, with one edge on blotting-paper to take up the superfluous fluid.

Next day apply to the film-side of the plate the film-side of a plate on which a réseau has been printed and developed; put these together in the printing-frame, and expose to the Sun for ten minutes or a quarter of an hour.

Take the plate from the printing-frame and put it, without washing, in the solution, and leave it until the réseau-lines are seen coloured in red, the plate being withdrawn from time to time for examination. Wash it to get rid of the excess of carmine and bichromate, and let it dry. The stars will then appear black, and the réseau-lines red. The operation is more successful if the plate has not been strongly alumed.

THE CAPE CATALOGUE OF ZODIACAL STARS.—This lately published work, which was mentioned at the last meeting of the R. A. S. (see p. 193), is not a catalogue of precision, but is a list of 2798 stars lying within about 6½ degrees on either side of the Zodiac which are to be used as reference points, when their places are accurately determined, from which the positions of planets, or in fact any other objects, can be deduced, the relative distances being measured by heliometer or other methods. The catalogue gives the places of the stars to seconds of R.A. and minutes of declination, with approximate values of the annual variation in each co-ordinate, so that it forms a working list for use in the observatories which undertake to make the necessary observations for determination of the accurate places. As a working list it might be found useful by some amateurs, although there are few such, who use a transit-instrument. It can be purchased of Eyre & Spottiswoode for half-a-crown.

A SPURIOUS COMPANION TO SIRIUS.—In our March number (p. 148) a note was published respecting some observations made by Prof. Barnard of small stars near Sirius; in the *Astronomical Journal*, No. 479, he announces that the star which he had called H at position-angle 281°, distance 40″·25, is not a real object, but is a reflection from Sirius. He says that there are two of these objects, so that reversing the telescope will simply substitute the one for the other. The fact that the telescope shows a similar object near Procyon at the same position-angle and distance makes it evident that these appearances are illusory. They did not exist prior to 1899, August 29, on which date the object-glass was taken from its cell and cleaned.

DOUBLE STARS. — Prof. Knorre, in *Ast. Nach.* 3632, gives measures of 26 Struve pairs ranging from 2″·0 to 4″·0 in separation. He used a double-image micrometer attached to a $9\frac{1}{2}$-inch refractor.

With the Lowell 24-inch Messrs. Boothroyd and Cogshall have made measures of 145 Burnham stars mostly of south declination. These measures were made in 1898, and will be found in the *Ast. Journal*, No. 478.

In No. 480 of the same journal is a list of 100 new doubles found by Prof. Hussey at Lick. These stars lie in the zone $-10°$ to $-13°$ Dec. Forty are under $1''$ separation, and the list ought to furnish some interesting pairs.

Forty-seven new pairs found in the zone $-2°$ to $-10°$ Dec. by Prof. R. G. Aitken are published in the *Ast. Nach.* 3635. These are the same class of stars as those in Prof. Hussey's list, 16 of them being under $1''$ separation.

OBITUARY.—Miss CATHERINE WOLFE BRUCE. We regret to see the announcement of the death of this generous lady, whose many and munificent gifts in aid of astronomical science have been frequently referred to in these pages.

THE Managers of the Royal Institution have awarded the Actonian Prize of 100 guineas to Sir William Huggins, K.C.B., F.R.S., and Lady Huggins for their work 'An Atlas of Representative Spectra.'

A TELEGRAM was circulated by the Kiel Bureau on April 10, saying that from four spectrograms Belopolsky has been able to confirm the *short* rotation-period of Venus. We need scarcely remind our readers that certain investigators have shown that Venus rotates in about 24 hours, whilst others affirm that the period is about 223 days.

ON account of the proximity of the total solar eclipse, and the consequent absence of astronomers from England, Visitation Day at Greenwich will not be on the first Saturday in June this year, but will probably be at the end of the month.

THE Bruce Gold Medal of the Astronomical Society of the Pacific has been awarded to Dr. David Gill, C.B., F.R.S., for distinguished services to astronomy.

BIRTH.—On April 17, at Lee, the wife of the Savilian Professor of Astronomy, of a daughter.

THE next Meeting of the Royal Astronomical Society will be on May 11, of the British Astronomical Association on May 30.

FROM AN OXFORD NOTE-BOOK.

THE eclipse which will have occurred before the next number of this magazine appears will not afford so many opportunities as usual of meeting our cheery American friends, for they and we can both see it by remaining more or less at home. Mr. Charles Burckhalter kindly reminds me that he still owes me two pots of jam, which he borrowed in Japan in 1896 and forgot to repay in India, and as we shall not meet this year he is afraid I must wait for Sumatra next year. This I am fully prepared to do, wishing him every success in the meantime with his beautiful buzzing screens, which we hope will give us a complete picture of the corona at all distances from the limb. Several interesting experiments are to be tried in America; I hear that the Yerkes Observatory party are to use a 60-foot lens, with a coelostat, which ought to give fine pictures. And Prof. Pickering has a comprehensive scheme for finding an intra-Mercurial planet, if there is one.

THE mention of intra-Mercurial planets recalls the name of Lewis Swift, and I was delighted to hear yesterday, through Prof. Barnard, that this veteran observer will in all probability be able to take a share in the observation of this eclipse, and is anxious to have another look for an intra-Mercurial planet. I call him a veteran, for he is 80 years old as we ordinarily reckon years, though he claims to reckon (by birthdays) as a youth of 19, having been born on February 29, 1820. He certainly has the stout heart of youth, for hear how he announces a sad misfortune that has lately come to him:—"I am sorry to inform you that my good eye has partially failed, and in consequence I have done no observing of consequence in nine months. I have discovered my last comet and nebula. This is a calamity to happen to a youth only 19 years old. Weather warm and pleasant." May we all meet trouble as cheerily at the advanced age of 19, or even 20! I have heard it said that the world has treated this hardworking courageous observer far from well financially, and he would be glad to sell his large telescope; it may conceivably be of some use to him to mention this fact here.

IN his own land Dr. Lewis Swift apparently has a great reputation among people of enquiring minds, and judging from a specimen of their letters which was published in the Pasadena (California) *Daily News* for March 29, and sent me by an amiable correspondent, I should say he must get considerable amusement from such admirers. The letter is as good a thing of its kind as I have ever seen, and I venture to give it *verbatim*. Those who read it will probably agree that though the man may not be "rich nor extravigant," his views are not undeserving of these epithets.

March 15, 1900.

Professor Swift Dear Sir my name is Joseph Stonham I live near Emporium Cameron Co Pa I am a carpenter by traid. I am not rich nor extravigant But I have peculiar views conserning the inside of this world my notion is that this world is holough inside like a cocoanut shel that there is openings at the poles say about a thousand miles in cicumfrense and there is currents of air passing in and out that keeps the earth pivioted on its axes also that the sun shines in at one end in June and at the other end in desember also that where it strikes square on the inside of the shel it heat up a belt around the inside the same as it does on the out side where the torid zone is causing day and knight summer and winter an the inside the same as on the outside also my notion is that the shel is about 500 miles in thickness and that would cause the holow inside to be about 7000 miles across which would give plenty of room for clouds and winds the same as on the out side and as for a moon the whole arch overhead would be a moon at night for those who might be living inside also my notion is that as the world is one quarter land and three quarters watter on the outside so it is 3 quarters land and one quarter watter on the inside in that case we have not seen only one quarter of the good farming land in this world yet. als when we go there we shal round rite around over the edge of the shell which is about 500 miles thick and get rite around into the inside and as we round over the edge of the shel we shal cum to a belt of 2 or 3 hundred miles in width which the sun shines square down upon our heads and it will be a warm climate that will bee our half way house then we shall have to pass another cold barer which I think will be rather less than the one on the outside and then we shal find the inside much like the outside with temprid zones and torid zones and a splendid climate and trees and vegitation much taller & more lucsurant than we have them on the outside that all these things may bee as I imagine I think there is very litle doubt how fine it would be to take a view in the inside with your great instruments at the oposit side and pehaps see a large steam boat sailing directly over your head about 7000 miles distant like you would be looking at the moon. in support of my theory I would ask you to examine the rings of saturn and see if they are not pretty near on the same plan and perhaps saturn her self is also below as I have imagined and jubter to. these notions seam far more sensible to me than to think that the world is all solld inside you see it looks far more sensible that the Lord would make it so on acount it would be so mch more useful. I would like to ask you what you think of it yours truly

JOSEPH STONHAM.

adress
East Emporium Camron Co Pa

BUT to return to the eclipse: it will interest many to learn that the Royal Astronomical Society are making it possible for their Assistant-Secretary, Mr. W. H. Wesley, to go to Algiers. Mr. Wesley has had more to do with drawing the corona than anyone that ever lived, but he has never seen it; all his acquaintance with it is derived from photographs. Now as Secretary S. P. Langley has recently remarked, those who have *seen* the corona know that there is a vast amount of beautiful detail which the photographs do not show, and it is eminently desirable that eye-observations should supplement photographic. If anyone is in a position to make the most of an opportunity for seizing on those features which do not appear on the photographs, I fancy Mr. Wesley is that person. He will further profit by the very great kindness of M. Trepied, who has offered him the use of a large instrument, possibly the equatorial coudé, for his examination of the coronal structure, instead of some small instrument such as could be taken out. This is a piece of international courtesy which has probably never been equalled, and does one good to hear of. We must try and keep the clouds away.

It is good news, too, that a coelostat is to be used in America; we who have used the instrument feel sure that it will be appreciated by others. It is exactly the thing for eclipse work. Talking of coelostats reminds me that in a lecture the other day I alluded to Joshua's commands to the Sun as a preliminary to explaining about a coelostat; and after the lecture one of my audience sent me the following extract from a Hotel Visitors' Book, dated 1854:—

Dr. Dodd stopped the night; perfectly satisfied.

(Below in another handwriting):

In Scripture we're told, that Joshua of old
"Stopped the day" when he slew the Philistines;
See all Kent in a fright, Dr. Dodd "stopped the night,"
Whilst he stayed to refresh his intestines.

The practice of inviting questions from the audience at the end of a lecture has almost become obsolete; but questions are sometimes volunteered in writing. A lecturer, who had patiently conducted his hearers through a course on spectrum-analysis (not myself this time), was favoured with the following enquiry:—"Will you kindly tell me where I can buy a spectrum, and what is the best to ask for? I want a good one for amateur work. Are they very expensive?"

As the springtime has come it seems as though we ought to have something in the nature of a poem, and I have been saving the following curiosity for some time to celebrate the opening of May with; the author's name is Merryvale:—

The Sun uprises with the day; gay May
Spreads all the treasures of the spring; ring, sing
Sweet warblers of the glen and hill; trill, fill
The woodlands with your merry note; float, boat
Where joy brings gladness to the eye; fly high
Blithe lark, &c., &c.

THE OBSERVATORY.

A MONTHLY REVIEW OF ASTRONOMY

VOL. XXIII. JUNE, 1900. No. 293.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 May 11.

Mr. E. B. KNOBEL, *President*, in the Chair.

Secretaries: H. F. NEWALL, M.A., and W. H. MAW (*vice* F. W. DYSON).

THE Minutes of the previous Meeting were read and confirmed.

Mr. Maw. Fifty-two presents have been received since the date of the last Meeting; among them are the following books:—Prof. F. K. Ginzel's 'Specieller Kanon der Sonnen- und Mondfinsternisse'; Vol. xliv. part i. of the 'Harvard Observatory Annals,' which consists of the Revision of the Harvard Photometry; and a book on the coming Eclipse of the Sun, issued and presented by the Director of the Lisbon Observatory. We have also received from Rev. E. Ledger a lantern-slide facsimile of Capt. Cook's original observations of the Transit of Venus, 1769.

A vote of thanks was accorded to the donors, and the lantern-slide was thrown on the screen.

The President. I have received a letter from Dr. Common, and the Council have asked me to communicate its contents to you. It is to the effect that the British Association has made a new departure in the Section "A," which has usually dealt with mathematical and astronomical questions. A Sub-section to Section "A" has been formed, which is to be devoted to Astronomy, and Dr. Common has been appointed President. He therefore asks me to give notice that any astronomical communication to the British Association should be sent to the Sub-section of Section "A."

Rev. A. L. Cortie then read a paper on the Duration of the Greater Sun-spot Disturbances in the years 1881–1899. The number of drawings of the solar surface made at the Stonyhurst

Observatory during the last nineteen years amounts to more than 3400, or, on an average, one for every two days. All the greater solar disturbances—such, namely, as attained an area of one-thousandth part of the Sun's visible hemisphere—have been recorded at least at some period of their life-histories. Recently all such disturbances have been charted, after the manner of and in succession to Carrington's well-known charts, and their life-histories written. This work has been done under the direction of Father Sidgreaves, and I have been able to use these materials in drawing up the present paper. I am also greatly indebted to the Greenwich volumes of sun-spot positions and areas in deducing the mean heliographic coordinates of the centres of the various groups. The paper consists of a Table and a Summary. The number of greater disturbances collected in the Table during the period under review amounts to 115. With regard to the disturbances, they are not necessarily confined to single groups of spots, but also include such as were due to several recurrences of outbursts of spots in approximately the same position. They are distinguished as "single" and "composite." The main problem I set before myself in drawing up the Table was the determination of the duration of such disturbances, so as to arrive at a mean value for the life of outbursts of solar spots. Assuming Carrington's value 25·38 for the period of a solar rotation, and arranging the disturbances in order of duration according to solar rotations, the greatest number of disturbances, 29·6 per cent., are found to remain visible for 1 to 2 rotations, and 24·3 per cent. for 0 to 1 rotation. Taking the mean duration of a disturbance from the whole number of disturbances discussed, the result is 56 days; while if we confine our attention to such as were born and died on the visible hemisphere, the result is 52·4 days. Hence we may conclude that the mean duration of a solar disturbance is slightly more than two rotations. The greatest duration of any disturbance was in the case of a composite disturbance, which consisted of four very large spot-groups, and thirteen others of smaller dimensions, succeeding one another in a restricted region of the solar surface, and which were obviously connected one with the other. This lasted 527 days, or for more than 20 (nearly 21) rotations, from September 25, 1891, to March 5, 1893.

Following the Table is a Summary, which contains a short account of the succession of the disturbances during the nineteen years, and points out the chief subjects suggested for future study. These points are further briefly re-stated at the end of the paper.

Some of these deal with what may, perhaps, be termed the "alternation" in sun-spot disturbances. All solar observers are aware that a large group of spots usually contains two greater members, a leader and a rearmost spot, smaller spots being interspersed between them. Sometimes one of these spots will develop, sometimes the other, a kind of alternating action being perceptible. A similar alternating action seems to belong to groups of spots in this wise, that a disturbance north of the solar equator is answered by its fellow in character and position south of the equator. One striking example is that which includes Nos. 98 and 99 of the Table, when two extended groups of spots answered one another, not only in character and position, but also in inclination to the Sun's equator. Furthermore a suspicion, which may perhaps be only the result of a set of curious coincidences, is suggested by the summary, that often one set of quiet rotations will alternate with an equal set of disturbed rotations. Again, in the years 1881 to 1886 September the number of great outbursts in each region north and south of the Sun's equator was almost equal, but since that date the preponderance has been markedly and remarkably in favour of the southern regions. This fact has also been noted in the papers presented to the Society from the Greenwich Observatory. If in the near future a period should occur in which the disturbance should be mainly confined to the northern latitudes, it would be a remarkable extension of the alternating action of these foci of solar disturbance. The mode of action, too, of the forces causing the disturbances which are associated calls for notice. At first it is intermittent, disturbances recurring in approximately the same position, with the intermission of one or more quiet rotations. Then the forces seem to gather strength, and the action culminates in a big outburst, to be succeeded by other intermittent recurrences of the spot-groups. This is true of the maximum periods of solar activity, for at a minimum the action is seemingly restricted to the intermittent mode of manifestation.

I may perhaps be allowed to mention a matter akin to the subject of the present paper, and that is as to the origin of the dark holes, first observed by Dawes, in the umbræ of spots. Observing a spot recently, I seemed to see a faculous veil extend from the bright interstices in the penumbra over the dark umbra, but leaving certain portions uncovered. The portions of the umbra covered by the veil were of a less dark tint than the dark patches or holes uncovered, and hence the distinction. However, I speak guardedly on the matter, as I have only observed the process once, and corroborative observation will be needed to justify any such hypothesis as to the origin of these so-called holes in the umbra.

The President. Father Cortie's paper is of extraordinary interest, and suggests many questions that we should like to have elucidated. There is one question I should like to ask him at once. He has stated in the case of a certain great disturbance, which lasted more than twenty rotations, that the spot-group extended over 30 or 40 degrees of longitude. What was the range in latitude of the group?

Rev. A. L. Cortie. The group lay in longitude 249° to 281°, and in latitude between 13° and 27° S.

Mr. Maunder. I should like to confirm your statement, Sir, with regard to this paper, that it is one of extraordinary interest. It certainly is so to myself, and I am very glad that Father Cortie has brought the subject up again lest any of us should fall into the delusion that there was not a very wide and rich field still to be worked in the direction of sun-spots. One of the most interesting points to my own mind that Mr. Cortie mentioned, was the recurrent outbursts north and south. He evidently connected the outbursts north and south of the equator. That is a phenomenon which I have noticed repeatedly during my 25 years' work at Greenwich. Further, we find that there are times when the Sun, as a whole, seems to respond to some influence, for disturbances will arise on a given day, or within a day or two, over a very wide region. There are two points which occur to me which are significant and should be carefully followed up, as they may give us a shrewd hint as to the depth to which sun-spots extend. We find in the formation of a certain type of group that the leader spot will move away from the bulk of the group at the rate of 7000 to 8000 miles a day. The rate is a very defined one. I do not think that you will find that the rate varies much, and it is evident that it has something to do with the depth at which the disturbance originated. There is another matter, and that is the average size for a perfectly circular stable spot, which is very much the same at one time as at another. From 10,000 to 20,000 miles in diameter is about the size of the circular stable spot. You may get spots which are very much larger, but if they are larger than 20,000 miles you will soon have them break up. I thought I would mention this in addition to the points which Mr. Cortie has brought forward as subjects for further study.

Mr. Newbegin. There is one question I would ask Mr. Cortie. He may have stated the point, but I did not quite catch it. When the southern hemisphere answers to the northern hemisphere, how long may the interval be before the answering outburst comes in the south latitude after it has appeared in the north latitude? Is it soon after or long after?

Rev. A. L. Cortie. I have instances put down in the Table. For example, No. 83 seems to answer No. 82. No. 82 was first seen in the year 1894 on September 5, and the answering disturbance started on November 26. No. 89 responds to No. 88. No. 88 was first seen on 1895 August 1, and died on November 2, while 89 was first seen on September 26 and was last seen on November 30. However, there does not seem to be any fixed law as regards the time when they succeed relatively to one another.

Mr. M. Horner. I am rather disappointed by Mr. Cortie's paper because I thought he was going to bring out something of the evident connection between these phenomena and the faculæ. I should like to ask whether his notes show if the spots have any relation to the faculæ or to the prominences?

Mr. Maw. I have been particularly interested by that part of Mr. Cortie's paper in which he refers to the variation in blackness of the umbra of sun-spots. I gather that Mr. Cortie considers this is caused by the spreading of more or less luminous matter in an irregular manner over the surface of the umbra. My opportunities for solar observations have been limited, but I have paid all the attention I could to this feature, and I have been led to an opposite conclusion to Mr. Cortie, and that is, that the mottling is caused by luminous matter below, and not upon, the surface of the penumbra. Last year I was watching a sun-spot in which a bridge was forming, and this bridge stopped at a short distance from the penumbra. At a later period I could trace this bridge as a very faint luminous streak beyond where it stopped, in fact quite across the umbra. I have seen many other less striking instances in which there were apparently veins of luminous matter submerged in the dark matter of the umbra, the spaces between these veins—which I have termed "submerged bridges"—forming the very dark spots or nuclei. The only difference between Mr. Cortie's views and mine is that to me the lighter portions of the umbra appear to be due to submerged luminous matter rather than to luminous matter situated on the top of the umbra. I do not know whether Mr. Cortie has traced these faintly luminous veins, but I have seen them well on a dozen or twenty occasions. They can only be observed under favourable circumstances, and I have never been able to see them except with a Dawes eyepiece, using a small aperture, which practically shuts out the whole of the penumbra. If there is anything like a glare in the field of view you cannot see these markings at all.

The President. Our Meeting is honoured this evening by the presence of Dr. Gill, from the Cape. The Society is always most happy to extend a cordial welcome to Dr. Gill when, after his periodic time of three or four years, he returns to perihelion. We have watched the notices he has been kind enough to send us for the Council's Annual Report of the progress of the great work he is carrying on at the Cape. We view with great interest the prospect of the work he is likely to carry out under the auspices of the very generous means which have been placed at his disposal. We are most happy to see him present this evening, and I would invite him to make some remarks about the work he has in hand, and that which he proposes to accomplish.

Dr. Gill. It is always to me a very great pleasure when coming to this country to meet this Society, and to receive the kind welcome which is invariably accorded to me. The President has asked me to give some account of the work at the Cape Observatory. I had intended to give some account of the McClean Equatorial, but I will defer that to the next Meeting, because I have had the misfortune to mislay some slides which I had prepared to illustrate my description of the instrument. Instead of describing the instrument to-night I will speak a little of the work we have been doing. I think the last time I was here the volumes describing the work of determining the solar parallax were not printed, but they have since been completed and distributed. Since that time the second and third volumes of the Cape 'Durchmusterung' have been completed. The second volume, I know, is in the hands of the Society, and the third volume is printed and will be in your hands in a very short time. I should like, in connection with that work, to say that all my life I have been very fortunate in the colleagues and friends who have worked with me. Without their assistance the great variety of work which has fallen to the lot of the Cape Observatory would have been impossible; and especially in connection with this 'Durchmusterung' I wish to mention the name of a man who, through thick and thin, and with very small material support, but with indomitable energy, has been the man who has made that work possible—I mean Prof. Kapteyn. When the project occurred to me, after making the photograph of the Comet of 1882, and it seemed that photography afforded the best means for the accomplishment of this work, but when the aid of the Royal Society failed me, I thought it probable that the completion of the 'Durchmusterung' would have to be the work of my old age, after my retirement from official life. Then came Prof. Kapteyn. He offered to do the measurement of the plates, and he has done it with a care and completeness which is beyond all praise. I know it is an unusual thing for one joint author of a work to speak in praise of that work, but Kapteyn's share of the work has been so much greater than mine that I can speak of it thus without feeling any loss of modesty. I have been fortunate in my present Secretary. My Secretary, Mr. Innes, has no official responsibility in connection with observations, but he has undertaken the responsibility of revising the work. He has devoted himself during the past year to hunting-up the origin of all discrepancies between the 'Durchmusterung' and catalogues of precision. In every instances it has been shown that those discrepancies are not due to the Cape work, but to the catalogue of precision, in a very great many cases to variable stars. He has discovered a good many variable stars, and those who will do me the favour to read the third volume when it comes out will find a preliminary list of the errors and discrepancies which have been discovered in the revision. Of course the revision is not complete, and the full detail of that will be found in the subsequent volume.

Another work which has been on hand for some time, and which is now all but through the press, consists of researches which have been made from time to time principally by myself, Mr. Finlay, and by a young Dutch friend, Mr. de Sitter. These investigations are the parallaxes of the southern stars, and the results are now in the press and will very soon be in your hands. They afford us a means of generalizing to some extent upon the constitution of the universe on various points in sidereal astronomy. We know from the researches of Prof. Elkin and Mr. Roberts, a South-African astronomer, that the mass of the star α Centauri is about equal to the mass of the Sun. I will now show you a photograph of the spectrum of α_2 Centauri compared with that of the Sun. In taking this photograph the middle part of the Sun-spectrum was shut out by a thin tongue of gold across the slit, and its place is taken by the spectrum of the star, and you see the spectra are identical. I think it is very important, with regard to our knowledge of the brightness of the Sun as a star in space, that we find the mass of α_2 Centauri identical with that of the Sun, and we find also the spectra of the two bodies identical, and it is not unreasonable to assume that the absolute light given by the two bodies is identical. That being so, we know the visual magnitude of α Centauri, and we know if we were to shift the Sun to a distance which would correspond to the distance of α_2 Centauri, we should have a star similar in appearance and light to α_2 Centauri, and we also find if we suppose the average parallax of a first-magnitude star is one-tenth of a second, which is very nearly the case, our Sun would appear a little brighter than a 5th-magnitude star. That I think is an important generalization in cosmical astronomy. Then with this fact before us, and with our knowledge of the parallax of many stars, we can come to another conclusion. Take, for example, that wonderful runaway star with an annual proper motion of 8½ seconds, which was discovered by Prof. Kapteyn and Mr. Innes. The parallax of that star has been determined and found to be about $0''\cdot3$. Its magnitude is 8·5, so that its total light energy is only ·003 of the total light given out by the Sun. Take a star like Canopus. The observations show a parallax of a few thousandths of a second of arc, which is less than the probable error. We have here a star about which we can form no opinion whatever as to its brillancy; but if we assume that the parallax of the comparison star is $0''\cdot01$, then the absolute brightness of Canopus would be about 20,000 times the brightness of the Sun. Or if we suppose the parallax of the comparison stars to be $0''\cdot05$, it would be 3000 or 4000 times the Sun's brightness. In other words, we have stars of known absolute brightness ranging from 3000 or 4000 times of brightness of the Sun to ·003 of the Sun. One star may be 100,000 times in absolute brightness greater than another star. Thus if we go parallax-hunting for the sake of finding large parallaxes, it is of little use being guided by the magnitude of the star. Mr. McClean has said, in his 'Stellar Spectra,' that the helium stars are apparently collected together in groups in certain parts of the heavens. In that case, and if the helium stars are collected in groups in space, it is most possible that the comparison stars which we have employed for finding the parallax of bright helium stars are also helium stars, and if the comparison stars we have used are only neighbouring stars of the same group we should expect to find very small parallaxes; and, as a matter of fact, in the heliometer observations I find none of the helium stars are stars which give a sensible parallax, a very striking proof of Mr. McClean's acumen. All stars of considerable proper motion do give a very sensible parallax.

As to other work which is of a less popular character we have been pushing on with preparations for fundamental observations, and have published a catalogue of 3007 stars. I am happy to say we are now on the point of approaching the construction of a fundamental catalogue which will be really valuable. I have just been down to Troughton & Simms' workshop and inspected the new transit-circle, which is all but complete, and I hope at the next meeting to have photographs and to point out its distinctive character, and the reasons why I believe it will give results of a more reliable character than can be obtained with the present instrument. That, however, is talking about the work of the future instead of work that has been accomplished. Another type of work that we have been busy about is the making of heliometer observations of the major planets to replace the meridian observations. I believe that heliometer observations will give the positions of these planets relative to neighbouring stars with a precision which is unapproachable by any number of meridian observations. The only point necessary is to secure sufficient co-operation by a number of different observatories in order to obtain accurate places of the comparison stars; and I am happy to say that a considerable number of observatories have promised their assistance, and have contributed the places of observations of comparison stars which will enable us to find places with a precision which has never been before approached in the history of astronomy. We have made considerable progress with the astrographic work. We have six ladies hard at work measuring, and they measured 37,000 stars during last year. The co-ordinates have been measured twice over, the plates being reversed in the micrometer; and if the measures do not agree within $0''\cdot6$ the observations are not considered complete, and the stars are again measured; but I should say it is very seldom indeed that a discordance of six-tenths of a second occurs. We find that the new instrument, of which a complete account has been published in the *Monthly Notices*, gives much more rapid results than I promised in the paper, for with additional practice observers have become much quicker in their working, and two ladies can polish off 400 stars in 4 or 5 hours with considerable comfort. The calculations have advanced, and I hope that, so far as the Cape is concerned, completion of the work will not be many years distant. Then, to leave astronomy, properly so-called, I come to the geodetic work which is being done on a large scale at the Cape. You know that the Geodetic Survey of Cape Colony and Natal has been published. That work was done by Col. Morris. My great desire—one of the great desires of my life—is to see the work of the Survey extending from the Cape to Cairo. There is an old Scotch saying which means that if you aim at something pretty large, and try hard for it, you may get at least a part; and I am pleased to say that with the sympathy of Mr. Rhodes and with the co-operation of the Chartered Company the work of that Survey is going on in Rhodesia very rapidly.

We have now a triangulation extending up through Rodesia to within 60 miles of the Zambesi, and I hope it will be extended beyond that by the end of the next observing season. We are stopped in our work there in the summer owing to the grass-fires of the natives, and then we are stopped in the wet season by fever; but by seizing the proper times in between those seasons it is possible to work for 5 or 6 months in every year, and the work is being assiduously carried on under Mr. Simms, with two other very energetic men who are assistants, and natives, and some Europeans for working the heliostats. During the bad season this staff comes down to the Observatory and its members are employed in looking after the instruments and in the reduction of the observations they have made. This embraces not only the triangulation, but a large number of astronomical observations. We have been hindered in the determination of longitude by the war. Everything was ready and the pillar was built, but the war broke out and everything had to be given up, but I hope by next season the line will be clear and the Boers all gone. I may mention—although perhaps it is a little premature to do so—that the question of extending the work northwards to the south end of Lake Tanganyika has been occupying my attention for some time. One has to look a long way ahead—as to where the money is to come from, and how the diplomatic difficulties are to be overcome where we have different nationalities; and such difficulties present themselves when we reach Tanganyika. I discussed the matter with the Secretary of the Royal Society, and he holds out the hope that the new International Association of Scientific Societies would be one of the best channels for approaching this subject. This is an Association formed amongst the principal European Societies, and the Royal Society has agreed that it will bring this matter of the arc of meridian along Tanganyika before the first meeting of this Association, which will take place in August. If we get it through there, its extension to Cairo need not be a great dream but a practical fact. There were certain diplomatic difficulties which arose in connection with the boundary of German South-west Africa and British Bechuanaland. The boundary now is diplomatically defined by the 20th meridian East longitude, and when I was last at home I was able, in concert with the German authorities, to arrange for a practical delimitation of that line. Both Governments were good enough to place the matter under my direction, and two Commissioners were appointed to do the field work. The arc of meridian will extend from Capetown to latitude 20° S., and will jump over to the meridian 30° W., and go on from there, I hope, to Cairo. Whether we shall be able to continue the 30th meridian is a question of the present war; but if the South-African Republic falls into the hands of the British Government we shall be able to carry it right down to the sea. These things all require great attention and that, together with an eyesight which is not so good as it was at one time, will prevent me undertaking so large a portion of the observing work as I have done; but still an older man, if he has the mind for it, may still find work for himself, and I hope for some years longer to carry on the work. (Applause.)

The President. Your applause has testified how interested you are in the statement which Dr. Gill has been able to make to us. No one can have listened to that account of useful work he is doing, and has done, at the Cape without being enormously struck with the energy which has been able to accomplish so much. I need not ask you to return your formal thanks to him because that has been proclaimed, but I should be glad to hear if there are any questions anyone would like to ask Dr. Gill upon the interesting statement he has made to us.

Mr. Newall. I should like to ask a question which occurred to me when I was looking at those very beautiful photographs of the spectra which were shown on the screen. Can Dr. Gill tell us what is the nature of the spectra of the separate components of α Centauri? I think I am right in saying the masses are about equal, and that they are equal to that of the Sun. It would be interesting to know whether their spectra are similar. One of the things which struck me was his remark about the distribution of the helium stars. To me the most remarkable fact is the suggestion by Mr. McClean that the helium stars are confined to one zone.

Dr. Gill. I think Mr. McClean suggests that there are groups in different parts of the sky, not that they are in zones.

Dr. Rambaut. I should like to take the opportunity of expressing the intense interest with which I have followed Dr. Gill in his account of the work in the southern hemisphere, and I think I am expressing the feelings of a great number of other Fellows of the Society in doing so. As the Society is aware, I have had the misfortune to differ on some very small points from Dr. Gill; they are, however, of a very small order indeed, being measured by hundredths of a second of arc. As such they are not large enough to prevent me from feeling anxious to add my homage to that of others, and to express my admiration for the splendid work he is doing at the Cape of Good Hope. Dr. Gill and I have had opportunities of expressing our views with the utmost candour, but I am not aware that hard knocks, when given without animosity, ever do much harm. Perhaps I may say that I have had some hopes (as Dr. Gill has been good enough to promise me a visit to the Radcliffe Observatory, and to give me the benefit of his opinion on the changes we are making there) that I shall have an opportunity of bringing about a *rapprochement.* No one can feel absolutely certain about the question of the probability of error. It is a somewhat troubled sea.

The same question which Mr. Newall has referred to—that of the helium stars—has also attracted my attention. I think it is a very interesting point. If I understood what Dr. Gill said aright, those stars of the helium type which he has measured have proved to have an insensible parallax, from which I gather that the comparison stars are also of the helium type.

Dr. Gill. They would probably be of the helium type. Until the 24-inch object-glass is in order it is impossible to determine whether they are, but we shall be able to find by photographic means whether they are helium stars or not.

Dr. Rambaut. I should like to ask Dr. Gill, before I sit down, whether, with the splendid spectroscopic equipment which Mr. McClean's generosity has placed at the service of the Cape Observatory, he will be able to sweep over the neighbourhood of these stars and distinguish between comparison stars of the helium type and others.

Prof. Turner. I should like to express my great admiration for Dr. Gill's work, and especially for his great scheme of the triangulation of the whole of Africa, which must be in all our hearts at the present moment. I suppose in dealing with schemes of this kind it is inevitable that Dr. Gill should pitch his standard very high. After duly recording my admiration, which I hope may be accepted, I want to quarrel with Dr. Gill a little over pitching the note so high that other people who want to be colleagues with him may not be able to attain it. My present work is concerned with the Astrographic Catalogue and the measurement of the plates in which eighteen observatories are co-operating. From the beginning I have taken some little share in trying to get Dr. Gill, who was one of the originators of the whole scheme, to whittle down the work to convenient dimensions, so that the other seventeen observatories may be able to tackle it. He has now told us what is approximately the progress of the catalogue in thirteen years. He originated the scheme in 1887; it is now 1900, and the preliminary work has been so much that he is now only beginning to measure the plates. Then he says that he has several ladies working with him and they have measured 37,000 stars in one year.

Dr. Gill. That has not been going on all the time. That is the number of plates that have been measured last year, but it does not represent the full year's work.

Prof. Turner. Can you replace that figure of 37,000 by another figure?

Dr. Gill. Well, perhaps 100,000.

Prof. Turner. Well that would modify what I was going to say. I was going to say that at the pace which he originally implied it would take the Cape Observatory 10 years to do the work. Dr. Gill would like to divide that by three, and therefore a great deal of what I would have said falls to the ground, but it must be remembered that there are several observatories that cannot afford to have so many ladies working for them. Many observatories have not got an instrument, and it would be, I think, a quarter of a century before the Catalogue is completed, and I would appeal to Dr. Gill to say whether he could not cut down the scheme a little more so that it would approximate to the scale we are adopting at Greenwich and Oxford, and so get the thing done more rapidly. In connection with his scheme for the heliometric observations of the planets—a most admirable plan for which I have the utmost sympathy—he is parcelling out the comparison stars for meridian observations. Would it not be a more economical plan to get this done by photography? Other observations in these days are most extravagant, and the principle ought to be adopted only for a few fundamental stars. By concentrating our attention upon a small catalogue of stars, and supplementing this by photography, I think we should get a very much better knowledge of the stars, and with less labour.

Dr. Gill. The variety of questions is somewhat embarassing. In regard to Mr. Newall's question as to what is the spectra of α_1 Centauri, compared with that of α_2 Centauri, I may say that the two stars are very nearly equal, but the spectra are very different. I have not got good specimens of both spectra, for α_1 is one of those which cannot be easily photographed. Prof. Rambaut has anticipated to some extent a point which is plain, namely, that one would desire if one could to have used stars of the solar type for comparison, but the heliometer determination of the parallax of the star is a very easy thing to talk about, but is a very laborious thing to carry out. I chose the plan of selecting the stars which were geometrically the most suitable for the purpose. In order to arrive at the spectra of the stars the best plan will be to photograph the spectra of all stars in the field with the objective prism. That will be done. Then we propose, also, to determine the parallaxes of those stars by photographic methods relative to each other. You cannot determine the parallax of a bright star by the photographic method. I may say with regard to photographing stellar parallaxes, it is a question of very great difficulty. The difficulty arises from a cause with which Prof. Rambaut is very familiar, the chromatic dispersion of the atmosphere.

I will now reply to Prof. Turner. He first asks that I should cut down the scheme, but I do not like to cut down the scheme. If his prediction that it will be 25 years before it is finished is true, it would not be a very bad thing. It is the monumental work of the century, and I think it should not be a question of time. To tie it down to a few years would stultify the work. It takes 10 years to make a Greenwich Catalogue of stars, so that by comparison 25 years would not be a long time to make an accurate catalogue on a so much larger scale. I may not live to see it, but I can afford to look at it from another world. (Laughter.)

The President. I can only ask you to renew your welcome to Dr. Gill, and on your behalf I offer him your warmest thanks for the remarks he has made.

The President. The Society is to be bereft of the services of those who carry on its work. The eclipse is an interesting event, but one of its results seems to be to deprive us of the services of our officials.

One of our secretaries has gone to Spain, Mr. Newall is going to Algiers, and our Assistant Secretary, Mr. Wesley, it is a pleasure to us to know, is also going to Algiers. Prof. Turner is going, as is also Mr. Maunder. The French Government Astronomer in Algiers has offered assistance to those who are going there in that generous manner that all who know him personally know he is capable of, and which we appreciate in the highest degree. M. Trépied, whom I have the pleasure of knowing well, is a right good fellow, and he has offered Mr. Wesley the use of one of the finest instruments in his Observatory, the *equatorial coudé*, and I hope that Mr. Newall will convey to M. Trépied an expression of the satisfaction with which that announcement has been received by this Meeting. I would now like to ask Mr. Newall and Prof. Turner to tell us the work they are about to undertake, and I am sure we shall all wish them every possible success in the observations they propose to make.

Prof. Turner. I shall try to repeat the work I did in the 1898 eclipse, which consists in the determination photographically of the brightness of the corona. It is a straightforward piece of work, but one evidently worth doing, and very little attention has been paid to it up to now. I think we ought to know the photometric value of the coronal light. I read a paper at the Royal Society yesterday giving the photometric determinations for 1893, and I am sorry that those for 1898 have not yet been published. Those for 1893 were especially interesting, as the observations were both visual and photographic. Sir William Abney was surprised when he saw the results. Another research, and slightly newer, is the work of photographing the corona in polarized light so as to get the value of the light of the two component rays. I have one or two subsidiary experiments to make, but the main work is that which I started in 1898, and hope to repeat this year, and I trust also in future years, so as to get a comparison which will be extremely useful.

Mr. Newall. I am going to Algiers, and am undertaking spectroscopic work. The main thing I attempted in India did not meet with that success which I had hoped from it. I hoped to determine the motion of the corona in the line of sight. The present eclipse is not favourable for that, because the totality is too short, and I am not making any effort to measure it on this occasion. I hope, however, to get photographs of spectra of the corona, trying especially to bring out the difference between the parts of the corona at different distances from the Sun's limb. I hope to get a photograph of the corona by the light of the green coronium, and further to make some photographs with a telescopic camera which I hope may throw light on the brighter and darker rifts in the corona, so far as polarized light is concerned.

Mr. Maunder. My wife and I propose to carry out, on just a little larger scale, our previous operations, and instead of the lens used in India we have a couple with the same aperture, but double the focal length. Our object will be to photograph the extensions again.

The President. We all wish the observers every success.

The following papers were announced and partly read:—

H. H. Turner. "On the Alleged Rotation of the Spiral Nebula M 51 Canum Venat."

John Tebbutt. "Observations of Minor Planets at Windsor, New South Wales."

Rev. A. L. Cortie. "The Duration of the Greater Sun-spot Disturbances for the Years 1881–99."

S. A. Saunder. "Notes on Measures by Prof. Barnard of Two Standard Points on the Moon's Surface."

W. Coleman. "Micrometrical Measures of Double Stars."

A. R. Hinks. "Diagrams for Planning Photographic Observations of Eros."

The following gentlemen were elected Fellows of the Society:—

Alex. Foote, F.S.A.Scot., 111 Warwick Road, Earl's Court, London, S.W., and Mall Park, Montrose, Scotland.

Prof. Ernest Lebon, 4 bis, Rue des Ecoles, Paris.

The following Candidates were proposed for election as Fellows of the Society:—

A. H. Baker, B.A., Head-Master, Basnett Road Board School (proposed by T. Lewis).

Wm. Hy. Colegrave, Little Tew, Enstone, Oxford (proposed by Duncan Forbes).

Guy François Comte Meredyth de Miremont, Orleans Club, St. James's, London, S.W. (proposed by Duncan Forbes).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The sixth Meeting of the current Session of the British Astronomical Association was held on Wednesday, April 25, at Sion College, *Mr. W. H. Maw*, President, in the Chair.

The election of seven new Members was confirmed and the names of five condidates for membership were read and suspended.

The President said that Mr. Maunder, Miss Bacon, and Mr. Crommelin had kindly promised to give some account of the work they proposed to do on the occasion of the forthcoming eclipse.

Mr. Maunder said that the programme which he and Mrs. Maunder proposed to try was simply a slight extension of that which they carried out in India in 1898. They would use a twin camera with two lenses, each 1½ in. aperture and 9 in. focal length, and make one exposure during the whole of totality. Then with the idea of getting the extensions, if possible, on a larger scale, they had a pair of rectilinear lenses of 18 in. focus, which worked also to the same aperture (1½ in.), and those they would expose also for nearly the whole time. If, therefore, they were fortunate they ought to get four photographs giving a considerable extension of the corona. They had a 4-in. telescope of rather short focal length with which they hoped, if possible, to get half-a-dozen photographs of the inner corona.

Miss Bacon said the American Expedition under her father's leadership would leave England on May 10, and they proposed to observe the Eclipse from Wadesborough, North Carolina. The party would consist of four ladies and four gentlemen. Mr. and Mrs. Nevil Maskelyne would (1) direct the telescopic kinematograph upon the corona throughout totality and (2) expose a long film in an ordinary kinematograph camera directed towards a chosen point of the landscape for a period commencing somewhat before, and terminating somewhat after, totality. Her father, the Rev. J. M. Bacon, would photograph the inner corona with a telescopic camera, 4·1-inch aperture and 60-inch focal length. Mr. G. Dixon would photograph the corona with a tele-photo camera or a 3-in. Dollond, or possibly with both. She (Miss Bacon) would endeavour to photograph the outer corona and extensions, using a clock-driven battery of four box cameras with sliding arrangement for changing plates. Miss Dixon would observe with a slitless spectroscope.

Mr. Crommelin showed a set of 20 slides illustrative of Algiers and its environs. He and his wife, he said, were going with Mr. Maunder's party to Algiers. They had a Dallmeyer tele-photo lens 1½-in. aperture and focal length adjustable. They would make it about 80 in., which would make the Sun's diameter about 8/10 in. With Cadett lightning plates and a ¾ sec. exposure they hoped to get the inner corona successfully. He hoped also to sketch a small portion of the corona.

Mr. Chambers alluded to the facilities offered by the Portuguese government to Eclipse tourists. He said that the party he had been organizing proposed to leave Southampton on the 11th May and be landed at Lisbon. From thence they would work their way up to Oporto, returning from Vigo on May 30th.

In reply to the President, Mr. Maunder said that he believed that at both Lick and Yerkes it was intended to take spectro-heliographs of the Sun and also to search for chromospheric lines.

Mr. Thwaites suggested that those who proposed to draw the corona should arrange previously what portion they would sketch, so that there might be no duplication of work, and that the sketching of the whole corona might be insured.

Mr. Evershed said that his work in Algiers would be to photograph the "flash"-spectrum. The principal instrument he proposed to take was an ordinary reflecting telescope, modified for the eclipse, and converted into a reflecting prismatic camera. By observing from a station on the edge of the shadow-track he hoped to get about 20 images of the flash-spectrum during the total phase.

A paper by *Prof. Turner*, in reply to the Rev. C. D. P. Davies's inquiries on the coelostat, was read. The writer said that (1) the axis need not be in the plane of the mirror, it was sufficient if it was parallel to it, but it was convenient for them to be near one another; (2) the suspension of the mirror could be quite satisfactorily arranged by three adjusting-screws; (3) observations near the ground were facilitated by a diagonal eyepiece; (4) with the heliostat the image of the Sun slowly rotated on the plate, with the coelostat it was absolutely fixed; (5) readjustment of the coelostat to the proper latitude could be readily effected by the use of a spirit-level and the declination circle. A single observation of the Sun then sufficed to adjust it in the meridian by making the Sun's declination, as indicated by the circle, agree with that given in the almanac. The Sun must not, however, be too near the meridian.

In a paper entitled the "Canals of Mars, &c.," *Mr. Holmes* expressed the belief that the peculiarity of sight that results in the duplicity of so-called canals, lakes, &c. is simply astigmatism of either eye or instrument, or both combined; that the word "canal" was used very loosely, that it was unfortunate that M. Schiaparelli was not content with discovering so many new features on Mars, but must also discover an entire new set of names for the old features; that the drawings of Schiaparelli and Perrotin were demonstrably incorrect, and that more must be allowed for the atmosphere of Mars than was usually done.

A paper by *M. Antoniadi*, on "Considerations on the Double Canals of Mars," was also read. The author set his own experience against that of Mr. Stanley Williams—who had affirmed that the plainer canals were continually visible to him—and stated that under no circumstances could he keep a Martian canal steadily before him; at best the canals appeared to him invariably by glimpses only. The remarks of Mr. Maunder on the "miraculous" character of Mr. Stanley Williams's observations were extremely interesting. He (the writer), too, had followed for years Mr. Williams's work and joyfully endorsed his brilliant discovery of dark spots on Saturn's belt, but the function of science being that of sweeping the miraculous from its battle-ground, and seeking to place every natural occurrence upon a cause, it might be stated from personal experience that small apertures suited these spots very well indeed, and that they could not really be missed by a careful observer, who, moreover, had paid much attention to kindred markings on a brother planet.

Mr. Maunder said he should be sorry if his use of the word "miraculous" with reference to Mr. Stanley Williams's observations were taken in any disparaging sense. He simply wished to refer to Mr. Williams's remarkable keenness of sight and acuteness of observation. With regard to the comparison of discordant drawings, there were two errors which they should be careful to avoid. If they had a large number of drawings of a given object, they would undoubtedly find that they differed in a great many details, and yet by carefully comparing them they could get at a very approximate idea of what was really before the different observers. They would not be justified in rejecting them on account of those differences, but, on the other hand, they most certainly could not assume that if two drawings differed, the object had changed in the meantime.

Mr. Thwaites expressed the opinion that with regard to the visibility of very small objects it was largely a question of illumination, and instanced the distances at which spider-threads could be seen on a frosty morning.

Mr. Tappenden remarked that it was always assumed that there was a large quantity of water on Mars, but he had never been able to understand why there should be any water. The planet was considerably further from the Sun than the Earth was, and the atmosphere was not nearly so thick, and yet our atmosphere was very few degrees above the freezing-point of water.

Mr. Whitmell did not think there was a large body of deep water on Mars. If there were, as pointed out by Mr. Taylor in a paper which he read before the Royal Astronomical Society, some time ago, an image of the Sun would certainly be formed, and no such image had ever been seen.

Capt. Noble said that Dr. Johnstone Stoney, arguing from the kinetic theory of gas, contended that the mass of Mars was too small to retain hydrogen. He (Dr. Stoney) had suggested that the polar caps of Mars were not snow or water at all, but carbon dioxide.

Mr. Whitmell gave a *résumé* of a paper he had written on "The Greatest Brilliancy of an Inferior Planet." The paper was mainly mathematical, but the writer mentioned that Mercury would be theoretically at its greatest brilliancy at the time of the approaching eclipse, and might, under the exceptional conditions which would then prevail, be visible.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The first afternoon Meeting of the present Session was held on Wednesday, May 16, at the Society's rooms, 70 Victoria Street, Westminster, *Dr. C. Theodore Williams*, President, in the Chair.

A most interesting paper was read on "The Wiltshire Whirlwind of October 1st, 1899," which had been prepared by the late *Mr. G. J. Symons, F.R.S.*, a few days before he was stricken down with paralysis. This whirlwind occurred between 2 and 3 P.M., commencing near Middle Winterslow and travelling in a north-north-easterly direction. The length of the damage was nearly 20 miles, but the average breadth was only about 100 yards; in this narrow track, however, buildings were blown down, trees were uprooted, and objects were lifted and carried by the wind a considerable distance before they were deposited on the ground. Fortunately the greater part of the district over which the whirlwind passed was open down, otherwise the damage, and perhaps loss of life, would have been considerable. At Old Lodge, Salisbury, the lifting-power of the whirlwind was strikingly shown by several wooden buildings being lifted up and dropped down several feet north-west of their original position. At a place 18 miles from its origin the whirlwind came upon a rick of oats, a considerable portion of which it carried right over the village of Ham and deposited in a field more than a mile and a half away.

A paper by *Dr. Nils Ekholm*, of Stockholm, was also read, on "The Variations of the Climate of the Geological and Historical Past, and their Causes." In this the author attempts to apply the results of physical, astronomical, and meteorological research in order to explain the secular changes of climate unveiled by geology and history.

Catharine Wolfe Bruce.

MISS BRUCE was born 1816 January 22. She was the daughter of George Bruce, the famous type-founder, whose work has stood the test of time and change, and is still in use at the present day; and naturally his daughter was interested in the art of printing—that "art preservative of all arts," as she was fond of quoting. Miss Bruce was an accomplished woman. She had made a study of painting, and was a painter herself. She knew Latin, German, French, and Italian, and was familiar with the literature of those languages. She wrote and published in 1890 a translation of the 'Dies Aræ.' She was a woman of the highest character, and had contributed nobly of her means to the cause of charity, of education, and of science. The George Bruce Free Library she built, established, and endowed. Her benefactions to the cause of astronomy are known all over the world, and many an astronomical project would have come to naught without her ready help. She corresponded with eminent professors in America and Europe, and was the recipient of distinguished honours for her interest and service. A gold medal was presented to her by the Grand Duke of Baden, and she enjoyed the signal distinction of having her name given to a newly discovered asteroid. Her pecuniary contributions to the science of astronomy amount to more than $200,000, and the recipients include names of individuals and institutions well honoured among astronomers in America, England, Germany, Austria, and Denmark.

For some years Miss Bruce had been confined to her room by an ever-increasing illness, and she was therefore personally scarcely known. She died at No. 810 Fifth Avenue, New York City, on 1900 March 13. (Compiled from *Pop. Ast.* 1900 May.)

The Solar Eclipse of May 28.

ANOTHER Eclipse has come and gone, observed it seems, from the preliminary reports, with complete success all along the line. The English expeditions, and in fact the European ones also, had ranged themselves almost entirely on the eastern end of the path of the shadow, through Portugal and Spain to Algiers. The following information as to their observations has already been received:—

At Ovar where the Greenwich expedition was located in a garden surrounded by houses and walls, the Astronomer Royal had a perfectly fine day, but the sky was somewhat hazy, and partially covered with light cirrus cloud, which may possibly affect the developed photographs, but the corona was perfectly visible to the eye, and was seen to be of the type associated with the minimum of the sun-spot cycle. Of the types shown on M. Hansky's plate, which was reproduced in the number of this magazine for 1898 February, the one which this year's corona was considered to most resemble was that of 1889, where the equatorial streamers are comparatively short and polar plumes are seen. This is confirmed by Prof. D. P. Todd, of Amherst College, who was in charge of the Lowell University expedition at Tripoli, and also considered the corona to resemble that of 1889. At Ovar the scene during totality was considered as bright as it was in India at the eclipse of 1898, when it was said that a newspaper could be read without artificial light. Venus and Mercury were seen, but no stars were obvious except Sirius. Three especially large prominences were noted in the lower part of the western coronal streamer.

From Algiers Mr. Wesley telegraphs that the corona was certainly of the minimum type, and considers that it resembled the predicted appearance given in the above-mentioned plate. Here also obscuration of the scene was not intense. Mercury and Venus were seen but no stars. Large prominences were also visible.

Cabled reports in the daily papers state also that cloudless sky prevailed in America. The Rev. J. M. Bacon was at Wadesborough, North Carolina, from where the report comes "Without a cloud." The Harvard Observatory expedition says, "Weather perfect, excellent observations." Several observers on both sides of the Atlantic state that the time of totality was shorter than was expected.

No news is yet to hand (May 29) of Sir Norman Lockyer, who was stationed at Santa Pola, Alicante, where Dr. Copeland also observed, but it is believed that they were favoured with fine weather. The newspapers have published accounts of what is termed "a regrettable incident," which we give with some reservation in the absence of corroborative evidence. It appears that Sir Norman Lockyer had arranged to entertain the French and Spanish observers at an afternoon *fête*, but through some misunderstanding the guests did not arrive until later than expected, which was resented by Sir Norman, who apparently considered it due to want of courtesy. It is said that amicable relations have been restored.

In the neighbourhood of London, light passing cloud prevailed throughout almost the whole of the afternoon, but the eclipsed Sun was perfectly visible at intervals. At Greenwich the time of first contact was not observed, but observations of position of the cusps and limbs were made with several instruments, and a series of photographs taken.

Interpolation *.

THIS is, we believe, the only complete and *exclusive* work on the subject.

The importance of the subject of Interpolation, and the immense economic advantages of its employment, are perhaps insufficiently realized and understood by practical astronomers, especially those whose work is necessarily isolated. The reason is not far to seek, for the great majority of mathematical tables and of astronomical predictions (such as can be tabulated at *regular* intervals of time) are already so far subdivided for their use that intermediate quantities can usually be obtained, with reasonable accuracy, merely by employing the method of proportional parts; that is, in the language of interpolation, none but first differences need be considered. Thus nicety of interpolation being seldom required by them, the subject escapes their attention in consequence.

But sometimes cases occur where (rightly or wrongly) it has not been thought necessary to carry out the subdivision to such a degree as to warrant the neglect of "second differences." In such cases three courses are open to an astronomer who has not studied the subject: he may neglect them, thereby introducing error which may or may not be systematic; or he may apply a correction for them which, according to his lights, may or may not bear any relation to the truth; or, lastly, he may waste valuable time in investigating the case on its own merits by the light of nature, which sometimes has a way of going out at awkward moments. If for these reasons alone, it would seem well to call attention to the existence of this branch of exact science.

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* 'THE THEORY AND PRACTICE OF INTERPOLATION: including Mechanical Quadrature, and other important Problems concerned with the Tabular Values of Functions.' With the Requisite Tables. By Henry L. Rice, M.S. Lynn, Mass.: Nichols, 1899.

But the promptings of gratitude itself are by no means to be ignored. For Interpolation is one of those remarkably self-contained engines which have done and are still doing an immense amount of useful work without useless clamour. It has been simply of incalculable service in the past in the minimizing of the mass of labour involved in the tabulation of mathematical functions for general use. As Professor Rice says (p. 79):—"All of the extended tables in common use, such as tables of logarithms, sines, tangents, &c., have been subdivided in this manner" (that is, by systematic interpolation), "at a saving of labour almost beyond estimation. In fact, interpolation has undoubtedly done more for mathematical science than any other discovery, excepting that of logarithms."

It is sometimes hinted on the subject of the *legitimacy* of interpolation, that while the various interpolation formulæ, or, more correctly, the theorems on which they are based, are capable of rigorous demonstration for integral values, they cannot be proved for intermediate values, which is just where they are wanted. Such assertions are a little misleading, and have here and there given rise to the belief that these all-important theorems have been carelessly accepted as working hypotheses by astronomers who knew well that analysis had failed to prove them. The fact is that it is known that every formula of interpolation absolutely *fails in general*—in other words, that the number of *à priori* cases where they happen to hold good is infinitely in the minority.

But since these admissions might at first sight appear even more damaging than the former statements (and might conceivably lead to futile research for colossal systematic errors in the *Nautical Almanac*), let us hasten to add that we use the italicized expression "in general" in its *à priori* sense. Thus, although the formulæ fail *in general*, it is customary so to choose the particular circumstances under which we apply them, that they hold good to the degree of accuracy required. To explain the cause of general failure, let us imagine a drop of ink to be placed on the rim of a penny, which is then to be rolled edgewise in an arbitrary manner on a sheet of paper, so that for each complete revolution of the coin an ink-mark is left on the paper. If the motion of the penny be "smooth," these ink-marks will form visible points here and there on an otherwise invisible curve, which would have been visible altogether if the whole rim of the penny had been inked. Now this curve gives *no indication whatever* of the path of the ink-drop in space, except the meagre details that it coincided at certain points with certain ink-marks on the paper. A more abstract instance of general failure is the case of the tabulation of the values of sin 0°, sin 180°, sin 360°, &c., from which evidently nothing can be inferred as to the sines of intermediate angles. The sphere of action of this principle is not confined to the region of exact science. If every evening at nine o'clock we found the same policeman in our kitchen, we should be tempted to ejaculate that he was *always* there, whereas reflection would show the insufficiency of data to justify such a conclusion. On this point of the legitimacy of interpolation, Professor Rice's remarks (p. 40) are happily chosen:—"Without certain restrictions or assumptions as to the character of the function and the interval of its tabulation, the problem of interpolation is an indeterminate one. Thus it is evident, *à priori*, that from a series of temperatures recorded for noon at a given station, it would be impossible to obtain by interpolation the temperature at 8.00 P.M. for a given day. If, *per contra*, the thermometric readings were recorded for 7.00, 7.10, 7.20, 7.30, ... P.M., it is highly probable that the temperature at 7.14 could be interpolated with accuracy."

These remarks of Professor Rice's occur in the chapter (no. ii.) on Interpolation, and this chapter, chiefly *synthetical*, is largely deduced from Chapter I., which deals with the *analysis* of Theory of Differences. Naturally, therefore, on reading these remarks one turns back to the first chapter, expecting to find there, if not a complete analysis, at least some indication of the general failure of interpolation. Now the only appropriate place for such indication is in Theorem VI. (p. 24); and it is just there, curiously, that we find lurking the one flaw (due to an oversight easily remedied) in a much-needed work, admirably planned and admirably carried out. The enunciation of Theorem VI. is as follows:—If the nth differences of a series of quantities (tabulated for equidistant values of T) are constant, the given quantities are the tabular values of a rational integral function of the form

$$F(T) \equiv \alpha T^{n} + \beta T^{-1} + \gamma T^{n-2} + \ldots .$$

Now this is undeniable, but cursory reading would lead one to imagine that the actual function originally tabulated must necessarily be identical in form with the particular rational integral function F(T) referred to, which is not the case, for all that can be said is that the actual function, whatever it may be, periodically coincides with F(T) at the points of tabulation. The actual function may, for instance, contain terms of the form

$$+ k T^{n-r} \cos\left(\frac{2M\pi T}{\omega} + \alpha\right),$$

corresponding to the term κT^{n-r} in F(T), where, of course, M is an integer and ω is the interval of tabulation. Then again, before the beginning of the proof, we read that "this proposition is the converse of Theorem V.," which is perhaps worse than no indication at all of the possible existence of periodic terms. Lastly, in the demonstration we find that the condition for constancy of nth differences, namely,

$$\begin{aligned}&\omega^n F^{(n)}t + B\omega^{n+1}F^{(n+1)}t + \ldots\ldots \\ &= \omega^n F^{(n)}t + (B+1)\omega^{n+1}F^{(n+1)}t + \ldots\ldots \quad (cp.\ \text{p. } 17),\end{aligned}$$

is interpreted as follows:

$$F^{(n+1)}t = F^{(n+2)}t = \ldots\ldots = 0,$$

whereas the only legitimate inference involving no assumptions extraneous to the enunciation is that for all integral values of s concerned (we symbolize for brevity),

$$\left(-1+e^{\omega\frac{d}{dt}}\right)^{n+1} F(t+s\omega) = 0,$$

which may be written

$$E^{s\omega}(E^{\omega}-1)^{n+1}F(t) = 0,$$

where $E^{s\omega}F(t)$ symbolizes $F(t+s\omega)$. The interpretation of this inference is of course neither more nor less than that the actual function, though it may involve, and in general will involve, periodic expressions, nevertheless is such that its particular values, as tabulated for the given equidistant values of T, coincide with those of some function

$$F(T) \equiv \alpha T^n + \beta T^{n-1} + \ldots\ldots$$

Chapter II. is written well and clearly and is nearly exhaustive. We find no mention, however, of Woolhouse's useful formula derived from the oblique set of differences immediately above Newton's. This formula, arrested at Δ_4, and with Δ_4 replaced by its mean with its *upper* neighbour, is not only equivalent to Bessel's (arrested at the same term), but has the additional advantage of using a single Δ_2 instead of a mean. This ingenious formula of Woolhouse's, modified as stated, was the basis of Godward's 'Interpolation Tables' for Intervals 12, 10, 8, and 6, which were greatly superior to any previous tables constructed for the purposes, and are still in use at the Nautical Almanac Office, chiefly for Intervals 12 and 6. Seeing, indeed, that the Moon's Hourly Places, as given at any rate in the British and the American *Almanacs*, are interpolated by means of Godward's Tables, it is surprising that no mention is made of Woolhouse and Godward in the articles on Systematic Interpolation, from which, by the way, one would gather that hourly quantities were derived from twelve-hourly by the laborious device of two successive bisections followed by trisection! British tax-payers would have something to say if this were the custom in the Nautical Almanac Office, where there is more systematic interpolation effected, it would seem, than in all the rest of the Queen's dominions.

The remaining chapters, dealing with derivatives and mechanical quadrature, leave nothing to be desired, and a most praiseworthy feature of the book is the Appendix on Symbolic Development, where the gist of some 200 previous pages is condensed into 6, forming a good object-lesson on the advantages of symbolism. We heartily recommend this, the only text-book on the subject, to the notice of the astronomical and the insurance worlds.

Nautical Almanac Office. T. C. H.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Stationary Radiants of Meteors.

GENTLEMEN,—

With reference to the interesting discussion which took place on this subject at the April meeting of the R. A. S., I fear that some of the facts of observation were not quite correctly stated as regards the Leonids and Orionids. In speaking from memory or from a mere cursory glance at my 'General Catalogue of Radiants,' it is, of course, very easy for anyone to mistake details.

It was mentioned that I claimed "a stationary radiant in the place of the Leonids which came into our system only 2000 years ago." But, as a matter of fact, I never, nor has any other observer, observed a radiant within the Sickle of Leo in October, December, or January, and such radiants as have been found there in November are all distinctly attributable to the great Leonid display, and were observed at or near the middle of the month. There is no stationary radiant corresponding in place with the Leonids for a long time before or after the occurrence of that display. It is true that a few radiants have been noticed there in the first half of February, and again during the first three weeks of April (G. C. cxvii.), but these may well represent two separate showers, and have probably nothing whatever to do with the well-known November system. Though I have watched the "Sickle" for hundreds of hours in autumn and spring, yet I have never found a radiant within it except the one formed by true Leonids at the middle of November.

As regards the Orionids, it was stated at the meeting alluded to that "the meteors begin to show in July or August, and go on until the following spring, having the maximum in October." The truth is that the special Orionid shower endures during the three weeks from October 8 to 29, as definitely stated in G. C.

pp. 216 and 290. There is another radiant about 4° N.N.W. of the Orionids, which is probably quite distinct, but it furnishes a stationary radiant (no. lxxv.), which apparently endures from August to January following.

As far, therefore, as my observations and comparisons enable me to judge, I conclude there is no long-continuing radiant in an accordant position either with the Leonids or Orionids, and the evidence in favour of this should be given sufficient weight in any discussion on the difficult and complicated subject of stationary radiation, which Prof. Turner has ventilated and explained with so much ability.

Yours sincerely,
W. F. Denning.

Bishopston, Bristol,
1900, May 7.

The Eclipse of Josephus.

Gentlemen,—

Those who have done me the honour to read the last edition of my little book on 'Remarkable Eclipses' will have noticed that I assign a date for this eclipse which is not the one usually given. Some persons even now take the eclipse mentioned by Josephus to be that which was total at Jerusalem soon after midnight on January 9, B.C. 1. But a careful investigation of the circumstances before and after the death of Herod the Great having shown that that event occurred several years earlier, Petavius suggested the eclipse of March 13, B.C. 4. This was a partial eclipse, only about half the Moon being obscured, and its time in Palestine was not long before daylight, the greatest obscuration taking place at $3^h\ 50^m$ in the morning. Not only, however, are its circumstances not very suitable, but it was long ago remarked that its date does not give time in the history for the death of Herod in the same year, as it is known that this event occurred not long before the Passover; hence Whiston, in accepting it, contended that Herod died in the following year, B.C. 3, which would give an interval as much too long as the other is too short. Greswell also took B.C. 3 as the year of the king's death, but as no eclipse of the Moon occurred in that year he suggested that Josephus (who wrote many years after the time) really means one which took place on January 20, B.C. 2, a slip of memory making him think that the eclipse occurred the year before and during the king's illness. This theory, however, is quite inadmissible, for the eclipse in question was scarcely visible in Western Asia at all; the middle of it, according to Dr. Grattan Guinness's 'Astronomic Tables,' fell at Jerusalem at a time corresponding to three minutes past 2 o'clock in the afternoon.

Prof. Stockwell, in the *Astronomical Journal*, vol. x. p. 188, contends strongly for the total eclipse of September 15, B.C. 5, as the one intended by Josephus. None could be more suitable in its circumstances, for its middle fell at Jerusalem at $9^h\ 45^m$ in the evening, and the date was probably about six months before that of the death of Herod. If we accept it, we are bound of course to conclude that the Nativity of Christ took place much earlier in the year than the traditional time of Christmas Day. But there need be no difficulty in this, for Greswell's arguments that the Nativity was in the spring have very great force and are not dependent upon the year assigned by him. It appears to me, then, that we may with considerable confidence take the lunar eclipse of Sept. 15, B.C. 5, to be the one referred to by the Jewish historian in the seventeenth book of his 'Antiquities.' It is the only eclipse of either Sun or Moon mentioned in any part of his writings.

Yours faithfully,

Blackheath, 1900, April 18. W. T. Lynn.

The Spectra of Algol Variables.

Gentlemen,—

Assuming the usual theory of Algol variables as double stars and the nebular theory of their evolution, some remarks occur to me which may be found worthy of a place in your columns. It is sufficient to apply them to Algol.

Algol ought to be rotating very rapidly in the same direction that the satellite revolves round it. The plane of rotation thus passes nearly through the Earth, and the lines in the spectrum ought to be broad because one side of Algol is moving towards us while the other side is moving away, under the influence of rotation.

But when the eclipse commences there is a change. The satellite will first blot out the side (or rather the greater part of the side) whose motion is toward us. The lines of the spectrum will accordingly become narrower, but will at the same time indicate receding motion. After the minimum this will be reversed. The part of Algol that is moving away from us will be eclipsed, and the lines, though somewhat reduced in breadth, will indicate approaching motion. At the minimum, as the eclipse will be annular, the lines will not be duplicated, but will be in their normal condition, the central portion being fainter.

At the crisis of the eclipse the satellite is nearest to us and Algol itself is farthest. Consequently Algol must have been receding before the minimum and approaching after it, which is established by spectroscopic observations. But the considerations which I have here urged may suggest that the *rate* of recess before the minimum and of approach after it has been over-estimated.

With regard to the periodical duplication of lines in some stars which are supposed to be similar in type to Algol, is it not possible that two totally unconnected stars which are nearly in the same direction may present a double-line spectrum when no telescope could separate them? But what, it will be said, if this duplication is periodical? I think the disturbance caused by a dark satellite of one of the two stars might suffice to explain this periodicity. If one star really got in front of the other, there ought to be a considerable diminution in the light; but it might get so near that with our present means we could not duplicate the lines although there was no actual eclipse. This explanation, however, might be invoked too frequently. Truly yours,

Dublin, 1900, May 5. W. H. S. Monck.

Periodically Recurrent Disturbances on Jupiter.

Gentlemen,—

The letter on pp. 215, 216 of the current number of the *Observatory* calls for a reply. For the reasons stated by Mr. Denning on p. 215, I purposely abstained, and do so now, from entering very fully into the subject, but I quite fail to understand in what way I have "imperfectly, if not incorrectly," referred to the facts. I expressly stated on p. 177 of your April number that Mr. Denning had discussed some* previous occasions upon which spots had been observed in the region of the N. temperate belt, and so far from ignoring the observations of Birmingham, Cook, Gledhill, Whitley, and others in 1870, and those of Barnard in 1890, it was precisely to these observations that I referred in stating that "the mere fact of spots having been shown by certain early observations and drawings really means very little in itself." The case of the spots which Mr. Denning states were observed by Prof. Barnard in October and November, 1890, seems to me to form a useful object-lesson in this connection. It happens that I observed a good many transits of the spots on the N. temperate belt in 1890, my observations extending into November of that year, and the spots in question were of the ordinary kind, and rotated at a rate approximately the same as that of the red spot. When the first spots of the true outburst made their appearance in 1891, not only was their aspect quite different and peculiar, but their rapid motion soon placed them in a different category altogether to those which had been visible in 1890, which latter spots, it is certain, did not form part of the great eruption of swiftly moving spots at all.

With reference to the latter part of Mr. Denning's letter, relating to the position of the red spot with respect to the hollow in the S. equatorial belt, the position is shortly this. My observations of the *spot*, published in *Astr. Nach.* No. 3596, are systematically earlier than those of the hollow as observed by others. These observations of mine were purposely made independently of the hollow, as in order to avoid being biassed I carefully abstained from looking at the latter as far as possible; but on one night, 1899, June 16, I found after recording the transit of the spot that the hollow was not yet quite up, and noted that the latter did not actually transit until 4^m later than my recorded time for the spot. Now here we have a real fact requiring explanation, and I suggest a very simple, probable, and adequate one. It is only necessary to glance through the numerous published sketches of the last 8 or 9 years to find almost every gradation of seeing, from the mere perception of the darker following border, through one-quarter, one-half, three-quarters, seven-eighths, to the complete spot. This indicates clearly, as I have sometimes noticed directly, that the spot of late years has usually been darker or denser on the following part, and from thence has faded off or become fainter towards the preceding end. If, therefore, some observers failed to perceive the faintest and most preceding portion of the spot, it is easy to understand how this systematic difference between us arose. The position is analogous to that of a faint extended nebula fading off on the edges, and probably many of us can recall somewhat similar differences of seeing, and where each observer, at any rate previous to the time of photography, would no doubt have been prepared to swear that the boundary or extent of the nebula seen by him was the true one.

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* Some, but not all, for there are other instances.

I am very far from claiming to possess any phenomenal or exceptional sight, but the words quoted by Mr. Denning, that "the spot itself was by no means difficult," stated the simple truth. Possibly the reddish tinge of the spot, and my eye is perhaps a little partial to red tints, may have rendered it easier to me than to others. It is well known that the red spot is much plainer photographically than it is visually.

Hove, 1900, May 5.

Yours faithfully,
A. Stanley Williams.

The Variable Star η Argûs.

Gentlemen,—

It would appear from Colonel Markwick's paper in the R. A. S. *Monthly Notices* for January last that his sources of information with reference to the above interesting variable are somewhat limited. In the *Monthly Notices*, vols. 31 and 37, a series of comparisons was published by me extending from 1854 to 1876. From 1876 to the present time I have made occasional comparisons, the complete results of which have not yet been published; and in 1888 I pointed out in your Journal, vol. xi. page 299, and in the *Astronomische Nachrichten*, Band 119, page 269, a remarkable increase of about half a magnitude in the star's light between 1887 April 23 and 1888 May 19, which announcement is unnoticed in the author's statement of magnitudes for the period 1884 to 1899 and in the closing sentence of his paper. As soon as I can secure the necessary leisure from other and more pressing engagements, I will carefully discuss all my observations from 1854 down to the present time, and with more improved data as regards the magnitudes of the comparison stars.

I am, Gentlemen,
Yours faithfully,
John Tebbutt.

Observatory, Peninsula,
Windsor, N. S. Wales,
1900, March 29.

OBSERVATORIES.

University Observatory, Oxford.—Owing to a change of date of Visitation Day at this Observatory, the Report of the Savilian Professor just received covers only the period of eleven months ending 1900 April 30. Sir William Huggins, who was appointed a member of the Board for the ten years ending 1900 December 14, does not wish re-appointment, as he finds the journeys to Oxford more than he can undertake.

The Report deals largely with the structural requirements of the Observatory. Prof. Turner begins with an interesting little history of the circumstances which led to the foundation of the Observatory, and shows that the construction of a house for the Director was evidently part of the original plan, but that the first Director, Prof. Pritchard, being too old to take an active part in observing, hesitated to ask for funds for a purpose not absolutely necessary. This reason for not building the house does not now apply, and Prof. Turner asks for means to work more efficiently. The new dome has not yet been supplied, for the reason that Messrs. Cooke & Son's works have been occupied more than they anticipated with the Transit-House for the Cape Observatory. A proposal is also made that electric light should be supplied to the Observatory, which could be done for a small sum; and the arrangements for the protection of the building from fire require revision.

The staple work of the Observatory has been, as in former years, the construction of the Astrographic Catalogue. The Royal Society continued the grant of £150 per annum for the fourth year (nearly completed) of the five during which it was originally proposed to ask for it; but, though the work is well advanced, it becomes clear that it cannot be completed in five years. At the present time 736 plates of the 1180 required are measured, and of these 705 are completely reduced. The corresponding numbers last year were 586 and 525. Copy for press has been begun, so that the Catalogue could be printed as soon as necessary arrangements are made with the University Press.

The De la Rue Reflector is in good order, as is also the Barclay Transit-Circle. The Report refers also to several papers communicated to the Royal Astronomical Society, emanating from the Observatory, and also to a paper on lunar measurements, communicated to the same Society by Mr. S. A. Saunder, to whom Prof. Turner lent a micrometer and afforded other help.

Yerkes.—The second annual report for the Yerkes Observatory is now to hand, from which we gather that the work is in full swing on the lines indicated in first report *. The details have already been published in several Bulletins, from the titles of

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* *Ibid.* 1900 March, p. 139.

which the programme of the work of the Observatory will be gathered:—

Bulletin No. 6.—Parallax of the Andromedæ Nebula. Spectrum of Saturn's Rings.
„ No. 7.—Spectra of Stars of Secchi's Fourth Type.
„ No. 8.—Opportunities for Students at the Yerkes Observatory.
„ No. 9.—Comparison of Stellar Spectra of the III. and IV. Types.
„ No. 10.—Period and Elongation of the Fifth Satellite of Jupiter.
„ No. 11.—Heat Radiation of the Stars.

NOTES.

Comet Notes.—We continue the ephemeris for Berlin midnight of Giacobini's Comet (*a* 1900) (*Ast. Nach.* 3636):—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
June 2....	1	3	53	28	36	June 22....	0	19	43	37	29
6....	0	57	49	30	11	26....	0	4	50	39	31
10....	0	50	40	31	52	30....	23	46	53	41	32
14....	0	42	9	33	39	July 4....	23	25	11	43	27
18....	0	31	59	35	31	8....	22	59	17	45	9

The comet is brightening, being twice as bright in the middle of July as it was at discovery.

Mr. C. J. Merfield has computed the orbit of Comet 1899 I. (Swift), using about 100 observations, the dates ranging from March 4 to July 15, 1899.

He deduces the following hyperbolic elements:—

		Probable error.
T	1899 April 12·977687, G.M.T.	0·000106
ω	8° 41′ 44″·8	2″·0
☊	24 59 10 ·9	1 ·3
i	146 15 31 ·1	0 ·7
q	0·3265681	0·0000014
e	1·000349	0·000004

A. C. D. C.

Minor Planet Notes.—The planet (433) Eros was detected April 26 at Arequipa, its R.A. being 1^m less, and its Declinati 8′ south of the position given by Millosevich's elements [*]. T would seem to imply that his value of the mean daily motion about 2″ too great, which is an unexpectedly large quantity. There is little chance of seeing the planet in this country before July or August; we therefore give no ephemeris at present.

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[*] A later communication from Arequipa states that the ephemeris comp from these elements is sensibly correct. The first statement was the resu an error in reading the ephemeris.

A. C. D. C.

THE VARIATION OF LATITUDE AND CONSTANT OF ABERRATION.—For some years past a series of observations has been in progress at the Columbia College University, New York, in combination with the Observatory at Capodimonte, Naples, for determination of the Variation of Latitude. There was a reason for the collaboration of these two institutions beyond the fact that they are widely separated in longitude, which is obviously of importance in such an investigation, in that Naples and New York have nearly the same latitude, and the advantage of this will be seen when it is stated that the latitude was determined by Talcott's method.

Talcott's method consists in choosing a pair of stars which culminate, one north and the other south of the zenith, at about equal distances from that point. The meridian zenith-distances of these two stars are measured, and since they are equal it is probable that instrumental errors have little effect on their difference. A little consideration of the geometry will show that half this difference added to the mean of the known declination of the two stars gives the latitude of the place, so that if the stations have the same latitude, the same pairs of stars can be used at both, and the difference of variation of latitude determined synchronously at the two places, which is the quantity sought for, is independent of the accuracy of the known star-places.

Prof. Rees has published at various times in the *Astronomical Journal* results of sections of the work done at Columbia College: in *Popular Astronomy* for April he gives in a most lucid form the results of the whole series. The simplicity of the method is apparent from the above description, and Prof. Rees's exhibition of the results is no less clear. The latitude was determined on 758 nights, between 1893 May and 1899 November; the mean of all was taken and then the differences of each from the mean. These differences being grouped to correspond to epochs separated by about 10 days, the means are taken and are plotted with time as abscissa, by which the variation of measured latitude is easily seen. The general correspondence in shape with the curve computed from Dr. Chandler's formula is obvious, although there are times when the curve of observations appears to be shifted bodily downwards; and also between 1896 and 1898 the observed epochs of maxima and minima follow the computed in time, which Dr. Chandler thinks is confirmation of his addition to the original theory that the ellipse in which the pole moves annually is not stationary, but that its major axis is gradually rotating. He further says that the observations indicate that the range of the fourteen months' circular motion of the pole has diminished since 1890, which fact was embodied in the formula some time ago.

It is impossible to do more than mention these small but interesting points in a short note of this kind, but it may be added that Prof. Rees's diagram makes it perfectly evident that the latitude of his Observatory varies periodically within a range of about 0″·4. We are not sure if the Naples series is completed, but we look forward to seeing the results.

The same observations differently treated furnish a value of the Constant of Aberration. Prof. Rees finds from the whole series 20″·464, with a probable error of +0″·006, which is not widely different from the previously determined values: the Paris Conference of 1896 May adopted 20″·47, but there have been several recent determinations giving values larger than 20″·5, so that no value can be considered at present definitive.

THE MOON HOAX.—In *Popular Astronomy* for May there is an article by Mr. S. A. Mitchell with this title, telling anew an old but remarkable story which we take the opportunity of repeating here in an abbreviated form.

In August of the year 1835 a series of articles appeared in the New York *Sun* purporting to be a description of Sir John Herschel's observations at the Cape: it will be remembered that Sir John landed in South Africa in January 1834. These articles begin in a *quasi* scientific manner, describing plans said to have been invented by the elder Herschel for constructing improved reflecting telescopes, which failed only because the size of the image was such that it was not sufficiently illuminated, and then go on to state that Sir John Herschel had evolved the idea of *lighting the focal image by artificial light* so that the faintest image formed in the focus of a telescope could be seen. Description was then given of an enormous instrument made by Sir John on these lines, and of the wonderful things he saw on the Moon by its aid—magnificent animals, creatures like men with wings, and so on. This fable appears to have met with considerable success; for one thing it considerably increased the circulation of the journal in which it appeared, and also it is said to have received some credence from Arago, who circulated the wonderful account through Paris. This last fact gives Mr. Mitchell a clue to the real authorship. He thinks that the actual originator of the "Moon Hoax" was Nicollet, an astronomer, who had left Paris in some disrepute, and did this, first, to earn some money, and, secondly, to entrap his enemy Arago into a foolish position.

THE PARIS TELESCOPE.—It must not be considered more than a coincidence that this note follows the preceding one. In recent publications of a popular kind there have appeared articles and illustrations relating to the telescope (50 inches aperture) now being shown at the Paris Exhibition, and the marvels that can be seen therewith. It is said that an image of the Moon 16 metres in diameter, and of Mars 3·70 metres in diameter, will be projected, presumably by the telescope, on a screen in a darkened room to be seen by a large audience. On this the following criticism in Publications Ast. Soc. Pacific, vol. xii. no. 73, seems very appropriate :—

"If such promises have actually been made it would seem to be worth while for somebody to point out their utter absurdity, since the daily press will certainly attribute them to astronomers. The area of the image of Mars, as above stated, is 8·8 times the area of the object-glass, through which all the light in the image must pass, and this number represents the ratio of illumination of object-glass and screen. In other words, the screen will be only about one-ninth as bright as a sheet of paper placed outdoors where Mars (and no other source of light) can shine on it. The image on screen will be further dimmed by loss of light in the apparatus, but whether it is a few dozen or score of times brighter or fainter makes little difference, as it will be equally invisible under all these circumstances."

THE NATURE OF THE CORONA.—A letter in *Nature* of May 3, over the signature of G. F. Fitzgerald, calls attention to an article by Herr Cantor in *Annalen der Physik* for this year, p. 462. Experiments are there described from which the author concludes that there is no absorption corresponding to the emission of light by a gas which is caused to radiate by electric discharge. Herr Cantor makes certain deductions as to the temperature of the gas which emphasize the difficulty of defining "temperature" in the case of a non-steady state; but the writer in *Nature* adds that whatever is to be deduced from the experiments they certainly lend weight to the suggestion that the corona is due to an emission of a similar character to that of a gas transmitting an electric discharge.

SIR DAVID GILL, K.C.B.—In the list of those on whom honours were bestowed on Her Majesty's birthday appears the name of Dr. David Gill, H.M. Astronomer at the Cape. Anyone who had the good fortune to hear Dr. Gill relate the tale of his work at the meeting of the Royal Astronomical Society, reported in this number, must certainly feel that among those honoured there was not one who deserved the honour more than he. In a recent American publication there may be read the address of the President of the Astronomical Society of the Pacific, made on presenting the Bruce Gold Medal to Dr. Gill, which tends to show the high esteem in which he is held on the other side of the Atlantic. In offering our sincere congratulations to Sir David Gill we feel that we are only echoing the sentiments of astronomers throughout the world.

WE have to acknowledge, with best thanks, the receipt of two excellent books on the eclipse — one from Signor Rodrigues, Director of the Lisbon Observatory at Tapada; the other from the Director of the Madrid Observatory. We mention these together for they are much of the same kind. The book from Lisbon begins with discourses on eclipses in general and the things to be observed on these occasions, and proceeds to the circumstances of the particular eclipse of this year, and concludes with numerical data and maps. The Madrid volume contains an excellent map of Spain and Portugal, with the circumstances of the eclipse shown thereon, which has been of the greatest service to us.

CONGRATULATIONS are due to Dr. A. A. Rambaut, the Radcliffe Observer at Oxford, who is one of the fifteen candidates selected by the Council of the Royal Society for election to Fellowship.

THE late Mr. G. J. Symons bequeathed to the Royal Meteorological Society all of his books, pamphlets, maps, and photographs, copies of which are not already in the library of the Society; also his Cross of the Legion of Honour, his Albert Medal, and other decorations, and a sum of £200.

EXCEPT for the exhibition by Dr. Isaac Roberts of his recently published book of photographs, astronomy was conspicuously absent from the Royal Society Conversazione on May 9.

THERE will be a very partial eclipse of the Moon, partly visible at Greenwich, on June 12, and an occultation of Saturn by the Moon on the evening of June 13.

THE next Meeting of the Royal Astronomical Society is on Friday, June 8; of the British Astronomical Association on June 27.

VISITATION-DAY at Greenwich this year will be on Tuesday, June 26.

FROM AN OXFORD NOTE-BOOK.

THE Note-Book has gone off on another eclipse expedition, this time to a "fixed observatory." We are installed at a little country inn of the most comfortable type, within a mile of the Algiers Observatory, and have at the moment of writing just put up our huts in the Observatory grounds, over solid stone piers for the instruments, which M. Trépied has most kindly had carefully prepared in advance. How often, if ever, has the track of totality included an observatory of any note? At present I have not the means of ascertaining. It would not surprise me to find that it had never happened before; it is at least so rare an event as to be regarded with something of suspicion apparently, for astronomers have taken comparatively little advantage of it. With all the conveniences of astronomical civilization close to the central line, with the most hospitable of Directors ready to do all he possibly can to help (I gave a striking instance of his open-heartedness in last month's notes), we are as yet the only party who have dared to reap these golden fruits. It must be that astronomers have observed eclipses so often from the desert that they regard the possibility of seeing one in comfort as "uncanny." However, we at least have no reason to complain.

ALGIERS is astonishingly close to London—you can leave London at 11 A.M. on Sunday, arrive at Marseilles 9 A.M. on Monday, leave at 1 P.M. the same day, and arrive at Algiers at 3 P.M. on Tuesday, to find stacks of geraniums, and to pluck the sweetest of oranges off the trees. The Algerian gardens *must* spoil people for any others. And one of the owners of such gardens tells us that if a goldfish be kept in every tank of water he will eat all the mosquito eggs (which are laid in water), so that there will be no mosquitoes to discount the pleasure of that particular garden. But as regards the journey, perhaps I have passed it over rather too lightly, for the 26 hours between Marseilles and Algiers may be considerably eventful; and it is quite worth while to collect one's thoughts a little at Marseilles before embarking on it, as we did. So that you should really start on Saturday from Charing Cross.

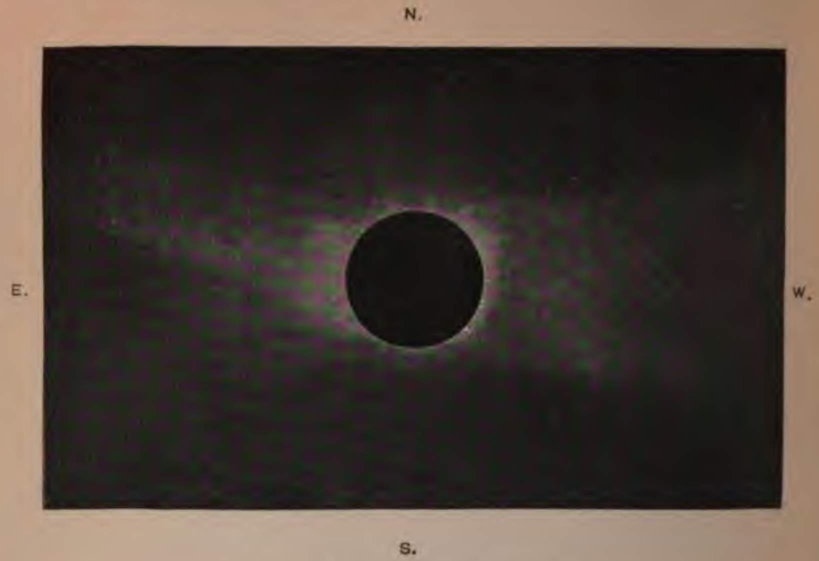
WE passed a pleasant day in Marseilles—ate *bouillabaisse* and climbed up to see Notre Dame de la Garde, the church which is such a conspicuous mark from the sea. Its immediate surroundings are rather desolate, steep slopes of gravel without a vestige of grass. On one of these barren slopes, in the full glare of the Sun, a couple sat picnicing with apparently as much enjoyment as though they were under shady trees in a grassy meadow. Presently the man's hat blew off and rolled down the slope about 50 yards; he followed it down with some difficulty, but his real trials began when he tried to get back again, and we watched his devious and slippery route upwards with the greatest interest. It was nearly as entertaining as the voyage of a mud-beetle we saw yesterday rolling his ball across the road, and finally trying to hoist it up the vertical face of the hedge-bottom; but the man *did* finally reach his companion, whereas the beetle was constrained, after being rolled over and over some dozen times in his fruitless endeavours, to give up his enterprise, and bury his precious ball in the ditch from which he had so manfully tried to escape.

AT the present date we are well established and at work. The work hung fire a little at first, because the instruments which had been sent round by sea only turned up about a week late, and we had therefore two days of unexpected idleness, of which, I am glad to say, full use was made. To-day it has been different. The cases and huts were delivered last night, and we have had a long hard day unpacking and setting up huts and things; one of those days of actual manual labour which are necessary on such occasions (whatever may be the assistance available) and always thoroughly enjoyable. Not the least pleasant part of the day was the *déjeuner* at noon, which our friends at the country inn aforesaid took a keen interest in providing, and which they served to us on packing-cases. Now we know what can be done in that way we have asked M. Trépied to lunch with us next Monday as a humble attempt to return his generous hospitality. We are concocting a programme to amuse him—soup in developing-dishes, let us say, measuring-glasses to drink out of, and chisels to cut up the viands. I hope he won't turn us out of the place afterwards.

From our station we get a glorious view over the bay below. This afternoon they have been having artillery practice in the bay, and with tolerably big guns, too, to judge by the sounds. We have begged M. Trépied to plead with the military authorities for a quiet day or two near the eclipse time; for although it is far from certain that such firing influences the weather, it is not at all certain that it does not. Many of those who were disappointed at Vadsö in 1896 blame the firing of the salute on a royal birthday for the clouds which gathered subsequently, and if there is even a chance that such explosions attract the clouds it is better to be without them. The Algiers Observatory is in a splendid position for settling this question by scientific evidence. From the regular meteorological records it would be easy to compare the weather just after artillery practice with the average, or with that on other days near, and get convincing proof one way or the other. But the result may vary with the locality, and if other meteorologists are near artillery, we should like to hear from them also.

Another anxiety we have is the wind. It seems to blow with intention on the top of this hill (1200 feet up directly above the sea), and we hear with mixed feelings stories how this roof has been lifted off and how that. We have tied down our canvas huts with ropes and pegs as tight as seems possible, and must hope for the best; but we have been hunting for that cipher telegram code arranged by somebody in 1898 which had a single word for "the whole camp carried away by a cyclone." When we read it originally it seemed humorous that anyone should have thought necessary to provide against such a contingency, but now it looks rather different.

There are, however, several advantages in being on the top of a hill. One is that the Observatory looks so fine from below—at any rate this Observatory does. There is a particular view of it, caught at just one point of the ascent, that is quite fascinating; I fear a photograph cannot do it justice, and am hoping for an artist to make the attempt. Another advantage is that the big telescopes get such fine views from above of objects below. This opens up a splendid method of amusing the casual visitor. M. Sy, the Assistant in charge of the equatorial coudé, has prepared a regular catalogue of the hour-angles and declinations of terrestrial points of interest which may be seen with the instrument, and most beautiful sights they are. There is also provided a fixed white screen, on which the Sun's image can be projected at any time, and the granulations of the photosphere and faculæ are shown on a large scale most beautifully. One quite longs to sit watching the screen and follow the history of a granule: it seems more than possible that this is the best way to do it. Visitors of all kinds are certainly well treated here, and the only fear is that when the mountain-railway up to the Observatory, which has just been sanctioned, is constructed, M. Trépied may have too many of *them*.



A Drawing made from photographs.

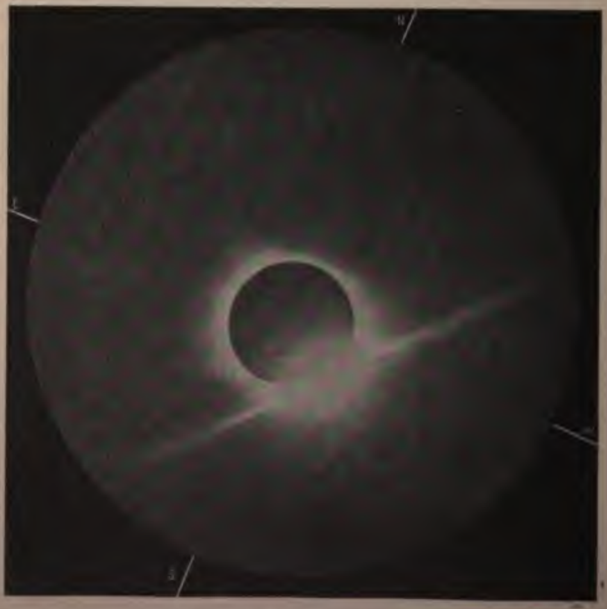


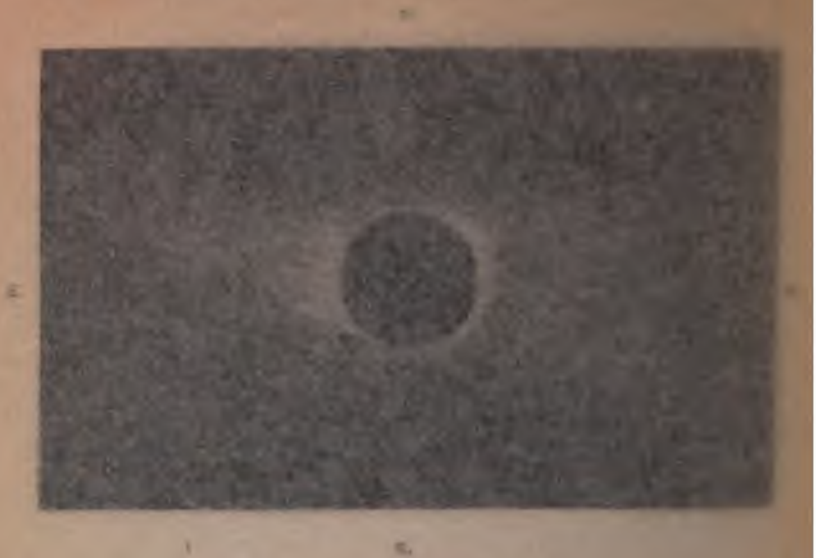
Plate exposed a few seconds before totality for 2 seconds.

THE CORONA, 1900 MAY 28. (See p. 281.)

[illegible]

[illegible] West [illegible]

[illegible] rather [illegible] The Canals [illegible] and [illegible] ster and S[illegible] this were [illegible] long gathered [illegible] in the south which [illegible] the [illegible]



A. Drawing made from photographs.

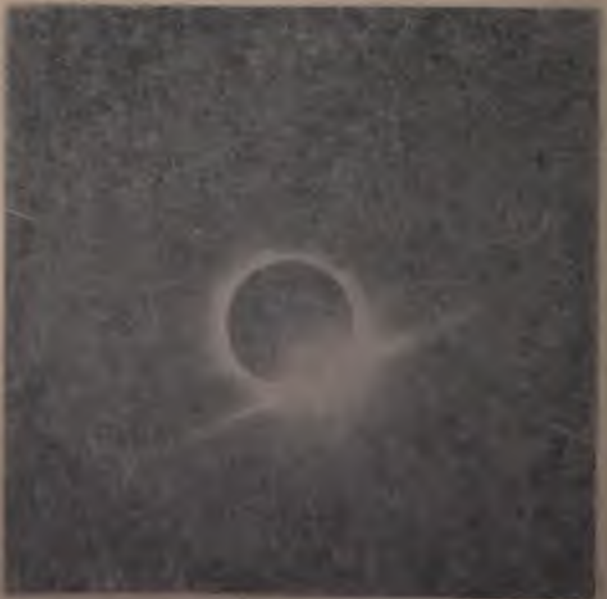


Plate exposed a few seconds before totality for a second.

THE CORONA, 1900 MAY 28. (See p. 281.)

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIII. JULY, 1900. No. 294.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 June 8.

Mr. E. B. Knobel, *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the last Meeting were read and confirmed.

Mr. Dyson. Fifty-two presents have been received since the last Meeting. Attention may be specially called to the present received from Mr. Wilson—a volume containing an account of astronomical and physical researches made at his observatory, Daramona, West Meath; also to a valuable and useful book by Prof. Wislicenus, 'Astronomischer Jahresbericht enthaltend die Literatur des Jahres, 1899.' We have also received a volume from Dr. G. W. Hill, 'On the Extension of Delaunay's Method in the Lunar Theory to the General Problem of Planetary Motion,' and Volume I. of the publications of the Yerkes Observatory.

Mr. Newall said that there had been received from Mr. Ernest W. Brown, of the Haverford College, Penn., a further chapter on the theory of the motion of the Moon. It had been ordered for printing in the Memoirs of the Society.

Mr. Newall then read a paper by *Mr. W. F. Denning* on a meteoric shower south of Corvus.

Prof. Alex. Herschel. The paper is a very interesting one in many ways, as it points out the occurrence of a rather remarkable shower in the springtime of the year. The Corvids to which it refers were seen in some numbers in April 1898, and two of the meteors were doubly observed between Leicester and Slough, and between Slough and Bristol, and their real paths were computed. They formed a shower of slow, long-pathed meteors with a very distinct radiant-point in the south, which one of the fortunately obtained real path determinations exactly verified; the other doubly observed meteor came from a closely adjoining, but rather weaker, shower-centre. Though I am sorry to hear from Mr. Denning that he is in such bad health that he can hardly engage much in meteor-observing work, I am glad to see he is yet able to illumine us by writing and compiling these papers, and that he promises to refer again to these matters. These far southern showers are interesting because they form a link between our point of observation and more southern stations, observers at Melbourne having been able to see some showers well, of which only the scattered, outside marginal meteor members probably appear above our horizon. One such group of radiants, which is referred to in the paper, is in Scorpio, where it is a centre of bright meteors appearing in May and June. They showed themselves, shooting up from a considerable belt of sky near the southern horizon, in May last year, at the beginning of the month. We shall hope for good accounts some day, from more southern stations, of that group of showers in the Scorpion's tail, which has only been well observed hitherto by Drs. Schmidt, in Athens, and Neumayer, in Melbourne.

The President. I am sure in returning thanks to Mr. Denning, who has been such an assiduous observer of meteors and has determined so many radiant-points, you will desire to express your sympathy with him in his indisposition. (Hear, hear.)

The President. We have several communications this evening relating to the late eclipse of the Sun. Perhaps it will come nearer home if we begin with that phase of the eclipse which was visible in this country. Mr. Newbegin has a paper, and I think he has pictures, which can be thrown on the screen, of the eclipse as visible here.

Mr. Newbegin then read a paper on "The Solar Eclipse of 1900 May 28th." He said:—The meteorological conditions in and around Norwich were much better than we were led to expect when the afternoon began. The first contact was not visible by reason of the clouds, but these soon settled more and more until that which looked nearly hopeless became quite a success. What cloud there was became rather useful than otherwise in making eye-observations. The Sun's cusp appeared very sharply cut out by the Moon. Venus was noticeable at the maximum, but no other star. I was able to expose a series of plates which I think will prove to be a fair representation of the eclipse as seen here. (Mr. Newbegin then showed slides on the screen and gave the periods of exposure of the various plates.)

The President. I am sure you will return your thanks to Mr. Newbegin for his interesting observations. We have present with us this evening Sir David Gill. At our last Meeting he gave us an account of his work which will dwell in our memories for a very long time. He promised on that occasion to give us some account of the McClean telescope, and I hope he will be kind enough now to redeem that promise.

Sir David Gill. I fear that to-night all your interests are centred, not in telescopes at the Cape, but in the accounts which we shall hear from observers of the eclipse, and therefore I think it will be well for me not to occupy too much time in redeeming the promise which I made at the last Meeting. Still, the McClean Observatory has peculiar features of importance which may not be without their interest to describe to you, especially as I believe that my friend Dr. Rambaut is erecting a similar instrument at the Radcliffe Observatory, and there may be points in it which may be useful for him to know in carrying out his scheme. I will ask to have first thrown upon the screen the general elevation or section through the centre of the north and south planes of the whole Observatory. Here you see something approximating to the general ground-plan of the equatorial. The mounting rests upon a very strong masonry pier, which goes down to concrete foundations on the hard rock below; there you see the dome and the walls of the observatory, and so far there is nothing very remarkable or unusual about the construction of the instrument. You see in the picture three telescopes, one for the photographic objective of 24-inch aperture and 24-feet 6 inches focal length; then the guiding telescope, in the ordinary sense of guiding which enables you to use a guiding star up to about one degree from the centre of the field; and then, partly covered by the 8-inch, is the 18-inch visual objective with its tube and without showing the micrometer eyepiece attached to it. Outside the object-glass and enclosed within a papier-mâché box is a jointed iron frame which carries the prism of the object-glass, which has an angle of 8¼ degrees. This prism can be very simply folded across the object-glass, or removed at the observer's pleasure, so that the telescope can be used for producing non-dispersed images of stars or spectra like those which are so well known in Mr. McClean's published work. We were compelled, on account of the impossibility of getting thicker glass, to have a prism of not more than 8¼ degrees, so it gives a much shorter spectra than in McClean's original pictures. When the object-glass is finally corrected for colour, as it is being done now in Sir Howard Grubb's workshop, we shall have the means of photographing spectra of considerably fainter stars than is the case in the McClean telescope. When we come to use an instrument of this sort, of this complex kind, it is absolutely essential for the comfort of the observer and for good observing to have some means of getting at the eyepiece in all positions of the instrument. We have therefore a rising floor, which consists essentially of a huge triangle of H-shaped steel, 18 inches deep, a large triangle, and the floor is rigged upon this on a sort of cantilever system. In order not to throw too much work on the hydraulic arrangements—for the floor weighs

12 tons—there are double counterpoise weights, leaving only a residual descending weight of half a ton on each rope. Then there is a three-stroke pump, which is turned by an electro-motor, and is available for use at any moment, and pumps water into what is called the hydraulic accumulator, or loaded vertical cylinder. The power is available for three different purposes—first for raising the floor, secondly for travelling the dome, and thirdly for winding the clock. The floor can be moved one-hundredth of an inch, or it can be moved 10 feet. The mechanism is very delicate. So far as practical working is concerned, one man can work the instrument, but it is always better to have a man to assist. The spectroscope is the work of Mr. Horace Darwin, assisted by Mr. Newall. I may say that Mr. McClean has left absolutely nothing undone. He has urged me to ask for everything necessary to make it perfect, and he has responded to my demands, by no means modest, to that end. (Sir David Gill described the details of the plans.)

The President. I should like to have invited some discussion on this interesting account Sir David Gill has given us of the magnificent observatory he has at the Cape, thanks to the munificent gift of Mr. McClean. The account Sir David has given us only makes us look forward eagerly for the results he will obtain with such splendid appliances. We are fortunate that the eclipse fell at such a date as to allow observers to return for this evening. We have great pleasure in welcoming here several of the observers who have been successfully engaged in Spain, Algeria, and Portugal during the late eclipse, and I will invite the Astronomer Royal to give us some account of the results he has obtained.

The Astronomer Royal. I am glad to have come back in time for this Meeting, but I feel some difficulty as to giving any account of the results obtained, because there has been an arrangement made that those results should be first published at a joint meeting of the Royal Society and of the Royal Astronomical Society. Therefore I must ask you to excuse me going into any particulars as to the results obtained. I may further say, speaking for myself and Mr. Davidson of the photographs of the corona, we have only just developed some photographs to show that we have obtained results, and I am happy to say we have got results from the eclipse, but I should not like to say anything further as to what they may show. However, it is a matter of common knowledge and of very great interest that the corona this year has conformed to what was expected, and has shown the characteristics of the corona at the minimum of sun-spots. I should not wish to sit down without expressing to this Meeting our thanks for all that has been done for us in our visit to Portugal. The Portuguese Government made every arrangement possible to facilitate the transport of our instruments as well as the observers, and we were very happy in having a friend of Mr. Maw's at Oporto, Mr. Frank Rawes, who made all arrangements for housing us at the rather out-of-the-way place where we set up our instruments. We are greatly indebted to the Portuguese Government and also to Mr. Rawes, of Oporto, for the great help they gave us in making our arrangements. Everything passed off very satisfactorily, and we return with very pleasant recollections of the welcome we have had from the Portuguese generally. I should wish particularly to express the great pleasure we had in visiting the Lisbon Observatory, and in seeing Mr. Campos Rodrigues and Mr. Oom, son of the late Director. We were very much impressed with the work which is being done, and we were very much struck with the great ingenuity shown in particular devices. I hope Mr. Rodrigues will be induced to describe some of these to the Society. We were especially taken with his arrangement for observing-chairs, most ingenious mechanical devices which struck us as worthy of imitation, and it will be interesting to astronomers in this country to know what has been done there. I mention this particularly because I must confess before I went to Portugal I had a very vague idea as to the activity of the Lisbon Observatory. We had not much information to go on, and I was very pleased to find it was in such an efficient state and doing such good work.

Prof. Turner. I was appointed by the Joint Committee of the Royal Society and this Society, along with a colleague who did not come, to go to Algiers. In place of that other colleague I am delighted to be able to say that Mr. Newall came, and although in some sense I was originally the leader of the expedition I should like to acknowledge that since then Mr. Newall took such a very leading share in it that our positions are now entirely reversed. With us was joined Mr. Wesley, not as an emissary of the Committee, but of this Society. He was to examine the corona from the point of view of the draughtsman, to see if we got everything on the photographs. An essential feature of his work was that M. Trépied kindly put at his disposal the best instrument he had in his observatory—the equatorial coudé. Mr. Wesley therefore saw the eclipse under what may be called the very best possible conditions. He will tell you himself what his verdict is. I would say one word as to the drawing. The corona was also photographed at Algiers, and I have rather hurriedly compared the position-angles of one or two points of the drawing with the position-angles on the photograph, not only of the prominences, but also of the points where the streamers join the limb, and I was startled to see the accuracy of the drawing that Mr. Wesley had produced. The accuracy may be described in this way: if you look at the face of an ordinary watch the division representing one minute is greater than the discrepancy between Mr. Wesley's drawing and the photograph. I was to take photographs of the corona with one half of the old double tube, and with the other half some special pictures in the polarized light of the corona. Mr. Newall also arranged an apparatus for attacking the question of polarization on quite independent lines, which was fitted up by him in half of the half of the double tube. I am glad to say both were a success. I took several plates during totality, and their value will be determined on measurement. We were very kindly assisted by several amateurs who came out from England, amongst others Major Kingsley Forster, who was good enough to undertake the rather ticklish duty of setting the Savart in my hut at a reading to be given by Mr. Newall, and the reading at which it was to be set was finally conveyed by word of mouth by Mr. John Potter, who ran between the huts with great celerity. It was a good job we did it in this manner, because certain people, who ought to have known better, made a lot of noise. We owe a great deal to M. Trépied. He was kindness itself, and it is difficult to express at this Meeting what we do owe to him.

Mr. Newall. Professor Turner has referred to the indebtedness that we are under to M. Trépied, and I feel I should like to emphasize that statement. We owe a vast amount of the success of the expedition to him; he forwarded all our aims and attended to all our wishes. We have come back with a great amount of material to consider. The work I undertook was mainly spectroscopic, and I had one large spectroscope with which I hoped to get the flash at the beginning and at the end, and a photograph of the spectrum of the limb relative to the flash. I come back with five photographs relating to the flash at the beginning of totality, and one of the corona which seems to be similar to the one taken in 1898 by Captain Hills, and those of the flash will give me more work to discuss than I am wishful to have put upon my hands. In addition to that I took some photographs with the view of trying to clear up points about the polarization of the light of the corona, and I have to thank Professor Turner for having included my apparatus in his faggot of telescopes. This is, I believe, the first time the Savart bands have been photographed, and these plates will be very serviceable in giving us information about the polarization of the coronal light.

Mr. Dyson. I have only just developed my photographs and cannot show anything on the screen, but perhaps I may be allowed to add a few words to what the Astronomer Royal has said in describing our expedition to Ovar. We were very much indebted to the Portuguese Government, and also to a very large extent to some amateur astronomers of this Society. I personally am indebted greatly to Mr. Atkinson. The eclipse-station at Ovar was in the garden of a house the soil of which was principally sand, so that there was no possibility of getting a good foundation. However, we mounted the instruments on our boxes filled with stones, and these acted admirably as foundations. My observations were made with two spectroscopes which Captain Hills took with him to India, and which he lent me for this eclipse. Ten seconds before totality the Astronomer Royal gave me the signal that totality was beginning, and we then began exposing for the flash. (Blackboard.) Twenty seconds after totality we exposed for the corona. I think all the photographs are quite successful.

Mr. Wesley. I was greatly indebted to M. Trépied for his extreme generosity in placing one of the finest instruments in his observatory at my disposal. I determined to examine the lower parts of the corona, to see if there were visible details that photographs have failed to show. I found that the equatorial coudé with a single lens eyepiece which Mr. Newall lent me, and which gave a large field of nearly a degree and a half, was most suitable for the purpose. I found that the definition was good all over the field. The image of the disappearing crescent of the Sun was thrown on the screen, and when the crescent became very narrow I removed the eyepiece and substituted the lens. The aspect of the corona was one of extreme familiarity and of a type which I knew very well, resembling that of 1889. At the same time the spectacle was more striking than anything I could have ventured to hope for. I looked carefully round the limb of the Moon, but entirely failed to see any very definite detail. The corona was very bright near the Moon's limb, rapidly fading into a pearly grey. In place of the interlacing structure which I expected, I saw only patches of greater intensity without definite outlines or edges, so that it was practically impossible to draw them. Mr. Evershed has kindly furnished me with the position-angles of the prominences, but I found I should lose too much time in trying to identify them, so I selected only the large prominence to the south-west, which served to orient the picture. I made rough diagrammatic sketches of the general form of the corona as well as I could, from which I made the drawing as quickly as possible afterwards.

Mr. Maunder. I am afraid I have not very much to say in addition to what has been already told you. As you may be aware, my wife and I went out on our own account to Algiers, and therefore we had neither very large instruments nor could we look for any assistance except what our own hands could give us. Still I think I have been able to carry the work which we started in India just a little step further. When we went to India we took with us the Waters' equatorial, which the Royal Astronomical Society was good enough to lend us, and we mounted on that, besides the Waters' camera, a little camera of our own with a lens of $1\frac{1}{2}$-inch aperture. Our scheme in India was to get exposures ranging from one second with the Waters' camera up to one equivalent to 1800 times that amount with the other lens, or six exposures in a geometrical series, with a common factor of $4\frac{1}{2}$. Each exposure we gave in duplicate, so we had twelve photographs in all. In this eclipse it was quite clear we could not carry on the series further in the same ratio, because totality was only one minute, and as our longest exposure with the Dallmeyer lens in India was 20 seconds, we could not carry on the ratio and give an exposure of 90 seconds here. I judged it very important that we should have our photographs in duplicate, and I thought the only thing to do was to get a second lens exactly similar to the one we used in India. We resolved to give 50 seconds exposure, the longest we could give safely, and we got also a second pair of lenses that had about the same aperture as the Dallmeyer but twice the focal length, and consequently they would give about one-fourth the same effective exposure. The eclipse in Algeria was, judging from the eye, nearly as clearly seen as that in India, but, it being a very much shorter eclipse, the sky was very much brighter, and, judging from a first inspection of our photographs, the shorter exposures were better than the longer ones for the extensions. I do not think we have been able to get the extensions further than we did in India, very likely not so far; but at the same time I think the photographs bring out quite clearly that the discovery we were able to make in India is verified on this occasion, namely, that the synclinal curves terminate in rod-like rays. I think that fact comes out clearly in these photographs as in the India ones, although I do not think we are able to trace those rod-like rays to anything like the same extent as on the three best photographs from India. Only those photographs which we took for the extensions have yet been developed. We have several more yet to develop.

Mr. Crommelin. As to time determination, I was lent a very good watch by the Hydrographer to the Admiralty, and Mr. Maunder had a very good watch from Messrs. Usher and Cole. These watches were compared frequently with one another and also with the sidereal clock at the Algiers Observatory. There was a difficulty in this comparison as there was no signal sent to the town, and the observatory is on a hill 1100 feet high; but the times are probably not more than half a second wrong. The duration of totality was 62½ seconds. The time given by the 'Nautical Almanac' was 67 seconds, so that there is no doubt the duration of totality given by it is too long. The American ephemeris gave four seconds less than the 'Nautical Almanac,' so that even its duration comes out slightly longer than the time observed on this occasion. Mr. Evershed was an unfortunate sufferer owing to the mistake in the 'Nautical Almanac.' He was two miles north of the southern limit as given by the 'Nautical Almanac,' but in the result he found himself outside totality altogether.

I tried with my three-inch telescope during totality to observe any structure I could within the region of the inner corona. There was some structure in the inner corona round the largest prominence, but I could not say for certain whether it was in any way connected with the prominence. I did not examine the surrounding corona to see if the structure continued there. There were two or three rays of the corona near the large prominence.

For the last nine seconds of totality I examined the whole corona with an opera-glass. It gave me the impression of resembling the corona of 1889 very closely.

Mr. Wesley. The only place where I suspected any kind of detailed structure was around a large prominence, but it seemed to me that the prominence lay in a kind of vacuity which I have indicated in the drawing.

Dr. Johnstone Stoney then read a paper on the cause of the shadow-bands seen during the progress of total eclipses of the Sun.

Mr. Franklin-Adams (showing photographs of the corona taken at Santa Pola in Spain). These photographs are shown, as they are the only ones yet arrived from the Santa Pola expedition. They were taken with a 3d Dallmeyer Group lens with tele-photo attachment. The Scottish Expedition, under the leadership of the Astronomer Royal for Scotland, sailed from Gibraltar in H.M.S. 'Theseus,' and returned in her some ten days later. The instruments from the Royal Observatory at Edinburgh were the 40-foot telescope, a 6-inch photo-visual objective made by Messrs. Cooke, and a prismatic camera. The Society will doubtless receive details of the work done with these on some future occasion.

My own instruments from Machrihanish Observatory were a 6-inch lens, $\frac{f}{4}$, by Cooke, a 3-inch Taylor-Hobson, two Ross lenses of two inches, and a 3d Dallmeyer. Besides these I worked a 6-inch of $\frac{f}{5}$ and a 4-inch of $\frac{f}{4}$ focus for Professor Barnard, of Yerkes. The three large telescopes were equatoreally mounted; I chose this battery and exposed in a great variety of ways, thinking that it would be a valuable experiment, although probably a self-sacrificing one, to discover how far it will, at the next eclipse, be possible to photograph planets and stars in eclipse-light. Three exposures were given with the chance of finding an intra-Mercurial planet, but for this the bright light was hopeless.

I hope to report later on the result of thermometer readings in tenths of degrees Fahrenheit.

Dr. Spitta showed three photographs of the corona taken with a landscape-lens at Algiers by Mr. Hodge*.

Mr. Maunder. One thing which struck me very forcibly about the corona was that in colour it was very unlike those of 1886 and 1898. That of 1898 seemed to me to be very much the colour of the electric arc—a sort of steel-blue; but this corona seemed to me of a very much warmer tint, a somewhat ivory colour. I should like to know if the same impression was produced on any other observer.

Mr. Franklin-Adams. If there was any colour at all it would be a combination of pearl and cream. The shadow-bands were described by two independent observers in two places as being like ripples on a pond.

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* See frontispiece to this number.

Prof. Turner. I saw all three eclipses that Mr. Maunder mentions, and I think he is right about the colours.

The Astronomer Royal. I have only seen two coronas, but it struck me that this corona was precisely the same colour as the corona that I saw in India in 1898, namely, a silvery white.

Mr. Newall. Silvery white describes the colour from my point of view at Algiers, but the Indian corona had more varied colour. Mother-of-pearl would describe the colour.

Capt. Hills. I understand that on this occasion the air was slightly hazy, so the colour would not be of a pure corona colour. It might be expected to be of a slight yellow tinge.

The President. We have had a great many most interesting accounts of the observations of the eclipse, and I will ask you to join in returning your thanks to the various observers. We shall look forward to our next Meeting in November, when we hope all the plates will be developed, and that we shall be able to have them discussed with more detail, and so reap the full scientific results of the expeditions.

The following papers were announced and partly read:—

W. F. Denning. "Note on a Meteoric Shower South of Corvus."

Ernest W. Brown. "Theory of the Motion of the Moon. Part III. Chapter VI."

Rev. W. Sidgreaves. "The Solar Eclipse of 1900 May 28, observed at Stonyhurst College Observatory."

G. J. Newbegin. "The Solar Eclipse of 1900 May 28."

Rev. S. J. Johnson. "Remarks on the late Total Solar Eclipse at Navalmoral (Spain)."

R. A. Sampson. "Description of the Durham Almucantar."

G. Johnstone Stoney. "The Cause of the Shadow-bands seen in connection with Total Eclipses of the Sun."

J. L. E. Dreyer. "The Partial Eclipse of the Sun, 1900 May 28."

Capt. W. Noble. "The Partial Eclipse of the Sun, 1900 May 28, observed at Forest Lodge, Maresfield, Sussex."

Radcliffe Observatory, Oxford. "The Partial Eclipse of the Sun, 1900 May 28."

E. W. Maunder and A. C. D. Crommelin. "Note on the Total Solar Eclipse of 1900 May 28, observed at Algiers."

Royal Observatory, Greenwich. "Observations of Capella as a Double-Star."

The following gentlemen were elected Fellows of the Society:—

L. N. G. Filon, M.A., Lecturer on Astronomy, University

College, London, Godwin House, St. Augustine's Avenue, South Croydon.

Forest Ray Moulton, Ph.D., Instructor in Astronomy, University of Chicago, U.S.A.

W. H. Pearsall, Higher Grade School, Dalton-in-Furness.

The following Candidates were proposed for election as Fellows of the Society :—

Sir Rajah Ankithum Venkata Jugga Row Bahadur, Mambram Estates, Vizagapatam, India (proposed by E. B. Knobel).

E. H. Shackleton, R.M.S. 'Tintagel Castle' (proposed by T. Lewis).

THE BRITISH ASTRONOMICAL ASSOCIATION.

The seventh ordinary Meeting of the tenth Session of the British Astronomical Association was held at Sion College on Wednesday, May 30th, *Mr. W. H. Maw*, President, in the Chair. Five new Members were elected and the names of three Candidates were passed for suspension.

The President read telegrams which he had received from Messrs. Maunder, Evershed, Downing, Howarth, Whitmell, Johnson, and Keatley Moore, all reporting fine weather and successful observations of the eclipse. He thought that these messages showed that a very good amount of work had been done. Prof. Todd was to be specially congratulated upon the success of his observations at Tripoli, because he had been most unfortunate in all his previous expeditions. He had spent a great deal of time and taken very great trouble to perfect his very ingenious arrangements for the automatic exposure of photographic plates, and if he had had good weather, as was reported, something very valuable might be expected from his observations.

Mr. G. J. Newbegin exhibited a diagram illustrating his observations of the partial eclipse, and *Mr. Hardy* and *Dr. Smart* exhibited photographs.

The President, *Capt. Noble*, and *Messrs. Seabroke*, *W. C. Tetley*, *Goodacre*, *Holmes*, and *Thwaites* all described briefly their observations. All the speakers agreed that the diminution of light was very small, that the limb of the Moon was singularly smooth, and that there was not the slightest indication of any lunar atmosphere, Mr. Thwaites describing the cusps as truly geometrical.

Mr. Wm. Anderson contributed a paper on "The Age of Sunspots in Relation to the Eleven-Year Period," in which he examined the records from 1891 onwards, arriving at the conclusion that if these 9¼ years might be accepted as typical so far as the ages of sun-spot groups were concerned, it would follow that groups reached their maximum age at about the time of sun-spot minimum, while their minimum age occurred only three or four years previously. The curve of spot-ages would thus appear to be not only periodical, but to have the same characteristic as the curve of spot-numbers, viz. the intervals from maxima to minima exceeded by several years those from minima to maxima. The author further stated that the table of percentages given brought out prominently the curious fact that groups between the ages of 16 and 25 days were almost entirely wanting. Only one group was found of an age between these two, and even in that instance it was open to question whether or not it died out in the further hemisphere, reappearing again as such groups so frequently did.

Capt. Noble said that it appeared to him that Mr. Anderson adopted a curiously arbitrary method of classifying his spots; they were new groups when he wanted them to be so, but sometimes they died and came to life again. He (the speaker) did not think it would be difficult to compile a table upon those principles which would prove anything, and referred to Bret Harte's well-known statistics by which he proved that it was infinitely more dangerous to go to bed than to travel on the railway.

A paper on "The Double Canals of Mars," by *Mr. A. Stanley Williams*, was then read, in which he referred to the recent discussions on the subject at previous meetings of the Association, and maintained that he had conclusively shown that some, at least, of the observed duplications must have been real, and probably so in most cases; and if that were admitted, even in a single instance, the whole ground was cut away from under those who argued in favour of optical or any other similar theory of duplicity, and it was no longer logical for them to hold that any particular case of observed duplicity must *ipso facto* be false.

Mr. Goodacre said he would like to read Mr. Stanley Williams's paper in print before saying much about it, but there was one point in Prof. Pickering's experiments which struck him. Prof. Pickering, as the result of his examination of Schiaparelli's maps, found that with an increase of aperture the space separating the canals was reduced in proportion—*i. e.* with 8½ in. aperture they were twice as widely divided as when using an aperture of 18 ins., the inference being that with 36 in. aperture the two lines would be merged into one, and this would perhaps account for observers using the Lick and other large telescopes not being able to see double canals on Mars.

A paper on "Diffraction Gratings and Diffraction Spectroscopes," by *Mr. A. Buss*, was next read. The writer first gave a short history of diffraction gratings, and enumerated the various methods of producing them which had been successively adopted, special reference being made to the replicas of reflecting gratings which had been so successfully made by Mr. Thos. Thorp. The differences between the spectra obtained by the use of prisms and diffraction gratings respectively were next explained, and the special features of diffraction spectras pointed out. An account was then given of a simple form of diffraction spectroscope which Mr. Buss had constructed for his own use and which was specially suited for attachment to small telescopes of small aperture, Mr. Buss's telescope being a 3-inch. This spectroscope, in addition to the usual straight slit, is provided with a curved slit for facilitating the search for solar prominences, and it is also fitted with a "rotator," an arrangement of mirrors which enables the object examined to be rotated in the field of view without turning the spectroscope. The paper was illustrated by a series of lantern-slides. After the reading of the paper, some of Mr. Thorp's reproductions of gratings were handed round, the President expressing the hope that at some future time Mr. Thorp might be persuaded to give them a paper on these gratings.

After a short discussion the Meeting was adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The second afternoon Meeting of this Society of the present Session was held on Wednesday, June 20th, at 70 Victoria Street, Westminster, *Dr. C. Theodore Williams*, President, in the Chair.

Mr. W. Marriott read a paper on "Rainfall in the West and East of England in relation to Altitude above Sea-level." This was a discussion of the mean monthly and annual rainfall for the 10 years 1881–90 at 309 stations, which the author had grouped according to the altitude of the stations above sea-level. The western stations were considered to be those which drained to the west, and the eastern stations those which drained to the east of the country. The diagrams exhibited showed that there is a general increase in the annual amount of rain as the altitude increases, and that the rainfall is considerably greater in the west than in the east, The monthly diagrams brought out prominently some interesting features, among which were: (1) that the monthly rainfall in the west is subject to a much greater range than in the east; (2) that in the west the maximum at all altitudes occurs in November, but in the east it is generally in October; (3) that in the west the spring months, April, May, and June, are very dry; and (4) that both in the west and east there is a very great increase in the rainfall from June to July.

A paper by *Mr. J. Baxendell* was also read, giving a description of a new self-recording rain-gauge, designed by Mr. F. L. Halliwell, of the Fernley Observatory, Southport. This rain-gauge, which the author believes approaches very closely to an ideal standard, has also the merit of being sold for a moderate price.

The Solar Eclipse of May 28.

The Meeting of the Royal and Royal Astronomical Societies to hear the Reports of the observers sent out under the auspices of the Joint Committee of the two Societies took place at the Royal Society's rooms on Thursday, June 28, at 4.30 P.M. The following summary of the proceedings gives in brief the main results of the eclipse. The Chair was taken by Lord Lister, Mr. Knobel occupying the Secretary's chair on his right.

The Astronomer Royal showed three photographs of the corona taken with the Thompson 9-inch object-glass and telephoto concave lens, giving a scale of 4 inches to the Sun's diameter; two of these, taken near the middle of totality with exposures of 6 and 12 seconds respectively, showed details of the inner corona and the equatorial streamers to a distance of about two diameters from the limb. The polar plumes were also well marked. The third plate, exposed near the end of totality for 2½ seconds, showed the chromosphere with a large prominence on the south-west limb. The Astronomer Royal also exhibited two photographs taken by Mr. Davidson, one with a 4-inch lens of 34 inches focus, the other with a special rapid lens of 13 inches focus, both taken with 3 seconds' exposure, and showing the extensions of the corona. Of these the picture taken with the longer focus appeared the better, and it was stated that with such a combination giving $\frac{f}{8}$ a longer exposure did not seem advisable.

Sir Norman Lockyer related that he had 120 officers and men of H.M.S. 'Theseus' at his disposal. He showed spectra of the chromosphere at different distances from the limb; also plates showing the spectrum-rings of the corona, the ring due to the green ray at 5303·7 and two others. It was pointed out that the rings are of totally different character from the chromospheric arcs, and have their greatest brightness at places other than where the chromospheric arcs are brightest. Photographs of the corona were also shown, taken by Mr. Howard Payn and by members of the ship's crew: one taken with a Taylor photo-visual lens of 4 inches aperture and 16-feet focus with 5 seconds' exposure, was considered good, the chromosphere being well marked all round the limb, and a large amount of detail of the inner corona well shown. A photograph taken with a Zeiss anastigmatic lens of 9 inches focal length with a Thorp grating mounted in front, the exposure being 40 seconds, shows a streamer—the N.E. quadrant—to a distance of 4½ diameters. No long extensions of the equatorial streamers could be detected by carefully-made eye-observations. Sir Norman stated that the corona was like that of 1878, not only in general form, but also in the minute structure of the inner corona, which was distinctly different to that seen in 1871. Also there were no bright lines, such as were seen in 1871, in the spectrum of the corona either in 1878 or in the present year.

Prof. Turner warmly acknowledged the kind reception and efficient help that he and the other observers at Algiers had received from M. Trépied. He explained by a diagram the principle of observation with a polariscope, and showed photographs of the corona in which the effect of polarization could be distinctly seen. He also showed photographs of the corona and stated that measures, not yet complete, had been made of its light, from which it was inferred that the corona of this year was comparatively bright. Prof. Turner made some introductory remarks on the merits of the coelostat as part of the equipment for eclipse observation.

Dr. Copeland showed photographs of the corona taken with the telescope of 40-feet focus and with a telescope having an object-glass of Iceland spar. The greatest extension of the corona was one diameter from the limb.

Mr. Evershed explained that he had gone, with his brother as assistant, to a station further west than Algiers, to a place which he thought, from the data of the British *Nautical Almanac*, was just on the south edge of the shadow-track, where it was expected that totality would last 30 seconds and that the chromosphere would have been seen all that time. Unfortunately the eclipse was not seen as total at this station. Some natives who were 500 metres north of Mr. Evershed saw a total eclipse; others, 500 metres south, said that the Sun did not wholly disappear. The edge of the shadow of the Moon was distinctly seen crossing the land north of the eclipse station. Nevertheless, photographs of the spectrum of the chromosphere and prominences were secured, and these were shown on the screen.

Mr. Newall showed spectra taken with a slit spectroscope at Algiers and with a Savart polarizing prism.

Mr. Dyson showed photographs of parts of the spectrum of the chromosphere taken with a slit spectroscope exactly at the beginning of totality.

Mr. Knobel congratulated the observers, but expressed a wish that wet collodion plates had been used.

Lord Lister also expressed his satisfaction at the results of the eclipse.

NAKED-EYE OBSERVATIONS *.

To observe this eclipse I went to Elche, near Alicante (Spain), a pretty town not far distant from the central line of totality.

At first I had some fears, because of ominous clouds and rain, but on the 27th the sky became perfectly clear and the eclipse was observed without the slightest trace of a cloud.

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* Kindly sent us by Prof. M. Moye, Montpellier University (France), 1900 June 16.

Besides photographs of the corona made in connection with the Montpellier University Expedition (results of which will be separately published) *, I made the following scientific observations specially on the shadow-bands and a tolerably exact sketch of the corona as seen to the naked eye.

Shadow-bands.—For these observations I was seated on a corn-thrashing floor, perfectly levelled and commanding a wide view of land. About three minutes before totality I saw the shadow-bands. They were regular, with the appearance of sinusoidal curves, not clearly defined, but greyish and faint on the ground. However, their intensity was sufficient to strike the attention of two Spanish policemen who were at my left.

The width of the bands was two inches; the distance between them was 1 or 1¼ foot. Their motion was uniform, and it seemed to me as quick as that of a man walking.

At first the motion was precisely in the direction E. to W.; but one minute before totality I saw a remarkable phenomenon (not observed before, I believe). Besides the first system already described, there suddenly appeared a second system of bands, showing the same general appearance, but their motion was distinctly in the opposite direction, viz., from W. to E.

I satisfied myself of the reality of the appearance, and it was testified to after totality by several persons who were near. I must add that the wind, during all the eclipse, was blowing with moderate force from the same point—E.S.E.

Appearance of the Corona.—To the naked eye, and as seen by an opera-glass, the Sun was perfectly black and dark, surrounded by a circle of a silvery white, almost dazzlingly bright, and resembling the classical drawings of an annular eclipse. This was the inner corona. To the right and left of the Sun were two immense streamers, of pearly tinge, tolerably bright, and fading away gradually on the edges. They were along the ecliptic, or nearly so; their length was two or three solar diameters; the western streamer was noticed very near Mercury, at east of the Sun. The form of the streamers was very conspicuous, each seemed as two curved rays with a central rift. At the Sun's poles were only short plumes, rather faint—a typical form of a minimum year.

No star except Sirius was seen, but Mercury was as bright as Venus at the beginning of the twilight, and it is needless to say that Venus was dazzling over our heads.

General Notes.—During totality the sky was dim, the blue having become an ashy grey. Round the horizon a large zone of a golden-yellow hue, with rosy and lilac streamers, produced a very artistic and beautiful view. The landscape appeared of a dull leaden grey; colour died out of the objects; the appearance was grand but rather weird. A dog seemed disturbed, and hens went to roost. The falling temperature as totality drew near was very striking, and the coolness was such as to become unpleasant. Thermometer (Centigrade) fell from 26° to 20° during totality.

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* Comptes-rendus de l'Académie des Sciences; Séance du 5 Juin, 1900.

Obscuration of the scene was not intense; all objects were distinctly seen; the divisions of a watch, or a newspaper, could be read without artificial light. The general illumination was very superior to that of the full Moon.

THE PARTIAL ECLIPSE.

This occurrence was well seen at Bristol, the sky being clear during the first half an hour. For the rest of the time there were some passing clouds, but the Sun shone through some clear spaces at short intervals and allowed the progress of the event to be followed to its close. A great interest was manifested in the event by the public, who were enabled to watch it with unusual facility, for the thin clouds which frequently covered the Sun enabled it to be viewed without smoked or coloured glass.

With a 4-inch Cooke refractor, power 65, I noticed two small groups of spots upon the Sun, separated by about 230,000 miles. The Moon encroached upon the westerly spot of the W. group at $3^h\ 12^m$, and on the similar spot of the E. group at $3^h\ 30^m$, by my watch, which was probably correct to within a minute. I made it a point to watch the individual spots narrowly, as the Moon's limb came up to them, to ascertain whether distortion or any other change of appearance occurred, but none was observed. Definition was excellent and the small spots were remarked as dark lumps on the Moon's edge at the instant prior to their disappearance, but their aspect remained perfectly normal until occultation. There was a little rippling round the Moon's periphery (this rippling is of course a constant atmospheric effect, which, though varying much in degree is never eliminated), but not sufficient to impair the image, which was very good.

One of the smaller spots—a mere dot—in the easterly group seems to have been intermittently visible. I saw it first at 1^h on May 28, but it had quite disappeared when I looked for it at 3^h. Yet at $3^h\ 30^m$ it was fairly distinct though small.

At the time of the middle of the eclipse an eye-estimate of its magnitude made it less than the proportion (about 7-10ths) stated in the almanacks. The air perceptibly became several degrees colder, and on taking a general glance round upon the sky and landscape the diminution of light was very obvious. The sunshine left had a sickly weird appearance, and the north-west quarter of the firmament looked very dark and lowering, as though a storm were coming down from that direction. W. F. DENNING.

Bishopston, Bristol,
1900, June 3.

We give as frontispiece to this number a drawing of the corona made from photographs by one who saw the total eclipse. Also a remarkable photograph taken by Mr. Hodge on a triple-coated

Sandell plate, and developed by Dr. Spitta. The exposure was made a few seconds before totality, and lasted two seconds. We are enabled to publish this by the courtesy of the Editor of *Photography*.—Eds.

The Present Appearance of Jupiter.

The low altitude of Jupiter at the present time necessarily constitutes a great hindrance to observation, and the conditions will be even worse next year. Fortunately conspicuous spots and markings are seldom absent from the disc of this planet, so that much can be done even under these unfavourable conditions. During the past two months I have obtained some fair views with my 6½-in. Calver reflector, definition being above the average, and these have revealed some rather interesting changes.

The most interesting, perhaps, of these changes is the increased plainness of the great red spot. During the past two or three years this remarkable object has been very faint, so much so, that the deep hollow in the S. equatorial belt opposite to it has been a much more conspicuous feature than the spot. But on the 20th April I was surprised and greatly pleased to find that the spot was much plainer than it had been. So obvious indeed was it, that it attracted attention *before* the hollow, the plainness of the latter being reduced owing to its being largely filled up by the intensified spot. Simultaneous with this increased plainness there has also been a reinforcement of tint. A reddish tinge has never been entirely absent, according to my observations, but the red colour has now become so intensified that the spot would be properly described as a red spot. The exact tint is pinkish or salmon colour, a little paler near the preceding end. The spot was observed to be in mid-transit at $14^{h}\ 40^{m}\cdot 4$ on April 20, the resulting longitude being $37^{\circ}\cdot 7$. My last observation of last year was on June 16, when the spot transited at $9^{h}\ 8^{m}$ ($\lambda = 31^{\circ}\cdot 4$), and comparing these two observations, the rotation period in the interval comes out $9^{h}\ 55^{m}\ 41^{s}\cdot 5$. The spot appears to me to be in nearly exactly the same longitude as the hollow in the belt. In 1898 it had distinctly followed the hollow, whilst last year, according to my observations, it must have, if anything, slightly preceded the hollow.

Another important change is in the colours of the two equatorial belts. Last year the S. equatorial belt was usually distinctly a little redder than the N. equatorial belt, the difference becoming less towards the latter part of the apparition. But now the N. equatorial belt is very considerably redder than the S. equatorial belt. The reddish colour of the latter is not now very marked, but the N. equatorial belt is of a really fine red colour, the colour of iron rust. These changes are exactly in accordance with the periodical variations in colour which I have recently described (see *Monthly Notices*, vol. lix. p. 376). The peculiar tawny or orange hue of the bright equatorial zone has now disappeared, the zone having a pale whitish appearance.

The low altitude of the planet is perhaps even more detrimental to the satisfactory determination of colour than it is to the observation of spots and other markings. It is therefore very desirable to have records of the colours of the belts by observers in more southern latitudes, and particularly by those in the southern hemisphere. What is wanted is not so much the exact tint or shade of colour as the degree or depth of redness of the different belts. Both the N. temperate and S. temperate belts seem to me more or less red at present, but I should very much like to get the fact of their having regained a red colour corroborated by more favoured observers.

There are a great many interesting and conspicuous spots visible now, particularly in the equatorial regions, so that the work of observing the planet is a fascinating and absorbing one, notwithstanding the unfavourable conditions, and the time so occupied is by no means wasted. A. STANLEY WILLIAMS.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Mr. Innes' Reference Catalogue of Southern Double Stars.

GENTLEMEN,—

An appropriate notice of this Catalogue of Double Stars has already appeared in the *Observatory*, but it seems worth while to call attention to a few points which may not occur to some readers.

The labours of most Southern Observers up to this time have been so desultory in character, their results so heterogeneous, and their papers so scattered, that, without a catalogue of uniform character such as Mr. Innes has produced, it was practically impossible to engage in double-star work and avoid duplication and mistakes. Now that Mr. Innes has brought together substantially all data accumulated by previous observers, noted the good points, pointed out the bad, and rejected the mistakes and duplications, which unavoidably arose, future observers will have an advantage which ought to render progress both more rapid and less difficult. Such a Catalogue would have been of the greatest service in my work at the Lowell Observatory, and in default of it some errors, due partly to lack of literature at my command and partly to the rapidity with which the work had to be done, inevitably were introduced. Mr. Innes has done me the great service to point out most of these errors, but a few of his criticisms appear to be misplaced. In general the impartiality and accuracy with which he has done his work is beyond all praise.

It is only by such digests that steady progress is made possible, and anyone who looks forward to the future development of southern stellar astronomy will see in this catalogue a basis which will serve the working astronomer for many years. The activity in this line, recently inaugurated at the Cape, is a commendable extension of the traditions of Sir John Herschel; and Dr. Gill and Mr. Innes evidently have due regard to the future as well as to the past astronomy of the Southern Hemisphere.

Who can doubt the increasing interest that will attach to double stars as time goes on, or question the importance of securing early records of the most important objects in the Southern Heavens? Probably many thoughtful persons will agree that henceforth the labours of astronomers upon the stellar systems must steadily increase, while in other lines of research, now relatively more fashionable or prominent, effort will be directed to the refinement of recognized data rather than to the explanation of phenomena still largely unknown and undeveloped.

Very respectfully,

Washington, D.C., 1900, Mar. 28. T. J. J. SEE.

The Annular Eclipse of 1820.

GENTLEMEN,—

Happening to be reading the other day Wordsworth's poem on the eclipse of the Sun in 1820, which he witnessed whilst on Lake Lugano in North Italy, I turned to the *Annual Register* for that year to see whether it contained any mention of the eclipse. It does, but some of its information is by no means correct. Its article on the subject thus begins:—"The solar eclipse on the 7th of September was the largest one that either has been visible in this part of the globe since April 1, 1714, or that will be so before that which will take place in 1847." The eclipse in question was annular, and the central line did not pass over the British Islands, so that the eclipse was only a large partial one there. I need hardly remind your readers that total eclipses of the Sun were visible in England on the 22nd of April (3rd of May, N.S.) 1715, and on the 11th (22nd N.S.) of May 1724. But that is not the only respect in which the above passage from the *Annual Register* is incorrect; indeed it would be difficult to find a sentence containing so large a number of blunders. The only eclipse of the Sun which occurred in 1714 was a very small partial one in the month of December; that of May 15, 1836 (when Baily's beads acquired that name), was annular in this part of the world, eleven years before that of October 9, 1847. It may also be remarked, with regard to 1714, that in that year April 1 by Old Style (still observed in England) was two days before New Moon, which occurred a little before midnight on April 3; it happened that April 1 was Easter Day by New Style, according to which the paschal Full Moon fell on the day which was called in England March 19, but by Gregorian reckoning was March 30. The second day after that being Sunday was Easter Day by that style, whereas as our March 21 was a Sunday, Easter Day fell on March 28, the Sunday after, and the Sunday following the day on which Easter was observed over the greater part of Europe—just as the Russians in the present year kept it a week later than we did. An eclipse on April 1, 1714, was impossible by either Old or New Style.

The *Annual Register* for 1820 affirms that during the eclipse not only Venus, but Mars became visible for a short time. It gives no authority for this statement, and, considering the great distance and feeble light of Mars when in that part of his orbit (he was in quadrature with the Sun at the end of April and in conjunction on the 21st of December), we may conclude that the observer, whoever he might be, was mistaken. The writer of the article seems to think there were two eclipses of the Sun in 1820, which was not the case. The central line of the annular one of which we are speaking passed near the Shetlands and then in a direction slightly east of south across the North Sea and Germany, so that Wordsworth was somewhat to the west of that line when he saw it on Lake Lugano. Yours faithfully,

Blackheath, 1900, May 1. W. T. LYNN.

P.S.—Since writing the above, I have referred to the *Gentleman's Magazine* for 1820. The account in the *Annual Register* is evidently taken from it, and, by a printer's error in doing so, the eclipse of April 1, 1764, is assigned to that day in 1714, fifty years earlier.

With regard to the statement that Mars was seen, Mr. Johnson points out to me that Baily says (*Memoirs of the Astronomical Society*, vol. i. p. 138), "Venus was seen by thousands of spectators with the naked eye: and I am informed that Mars also was visible to many." This, however, is too vague; Mars, if visible at all, could only have been so to those who knew where to look.—W. T. L.

Periodically recurrent Disturbances upon Jupiter.

GENTLEMEN,—

I fear that little good will be effected by continuing this discussion, for the question involved is one which must depend upon the facts of future observation rather than upon the various interpretations which different observers may put upon existing records. My views differ materially from those of Mr. Williams, and not only upon this matter but upon some other important details of recent planetary observations.

In his letter published in your June number, Mr. Williams says nothing in support of his view that the period of the disturbances in Jupiter's N. temperate belt is nearer 12 than 10 years. I should now be disposed to think that, if the phenomena concerned are recurrent at pretty regular intervals, the cycle is less than 10 years. J. H. Schroter drew some remarkable spots in the N. temperate region on April 7, 1792, which, compared with the outbreak of October 1880, probably comprised 9 returns of 9·8 years.

During the present apparition of Jupiter the N. temperate belt has grown much darker, and I trust that observers will watch it closely during the next year or two for the reappearance of the spots which marked it in 1869–70, 1880, and 1890–91.

Mr. Williams refers to the difficulty of seeing feeble nebulosity as a parallel case to that afforded by the faint preceding end of the red spot, but the two things are essentially different. On a really good steady night the faintest side of the spot is sharply bounded from the brilliant material running along its S. and S.W. borders. In fact the outline of the spot has a very definite aspect[*] in highly favourable moments, though it is faint enough to be practically obliterated in the bad or indifferent seeing which often prevails. But in the case of outlying nebulosity it is sometimes exceedingly diaphanous and quite indeterminate as regards its exact area, for, no matter what the character of the definition may be, no eye can assign the precise limits of an object which apparently fades off into the general tone of the sky ground.

Having seen (apart from my own) many drawings of Jupiter's red spot in 1899, and compared a large number of descriptions, I am bound to conclude that Mr. Williams's results for the same object are affected by some individuality, and that the abnormal length and position which he ascribes to the spot are not in accordance either with the records of other observers or with the facts of Nature.

Yours sincerely,
W. F. DENNING.

Bishopston, Bristol,
1900, June 7.

The 'Elémens d'Astronomie' of Nicolas Fizes.

GENTLEMEN,—

The above work was published at Montpellier in the year 1689. Lalande says of it, in his 'Bibliographie Astronomique' (p. 321) that "L'auteur prétendait que la terre ne tournait pas sur son axe, parce qu'il faudrait qu'elle fût cylindrique." In Zach's 'Correspondance Astronomique' (vol. iv. p. 612) the editor indulges in the following sarcastic remarks on this extraordinary notion:— "Si le célèbre médecin *Fizes* n'était pas plus habile dans sa science, que ne l'était son parent en astronomie, nous plaignons ses malades, quoique tous morts depuis longtems soit naturellement soit artificiellement . . . . il démontre mathématiquement dans son ouvrage, que la terre ne pouvait pas tourner sur son axe, parceque, dit-il, qu'il faudrait pour que la terre puisse faire ce mouvement qu'elle fût—*cylindrique*!" He had before suggested that Fizes was "probablement parent du célèbre Médecin de ce nom," and the astronomer Nicolas Fizes was in fact the father of the physician, whose name was Antoine Fizes. Estève gives a short account of the son's life in 'La vie et les principes de M. Fizes, pour servir à l'histoire de la Médicine de Montpellier,' which appeared at Amsterdam in 1765. Here we shall only refer to the particulars mentioned of the father, Nicolas Fizes. He was a native of Frontignan, and went to Montpellier in order to study at the college founded there by the Jesuits in 1629. That is the only date with which Estève deigns to furnish us; but we know that the son Antoine (who was not the oldest) was born in 1690. He made himself master of a great number of languages, and also studied law, but abandoned that pursuit for mathematics and physics, and being employed as a consultant by the generals in the war, the king (Louis XIV.), when he founded a chair of mathematics and hydrography at Montpellier, nominated him professor in those subjects, it being arranged that he should spend half the year at Montpellier and the other half at Frontignan, which is near Cette and on the coast, about twelve miles south-west of Montpellier. His book on astronomy, as already mentioned, was published in 1689. He tells us, in the introduction, that its aim is not that "d'expliquer l'Astronomie à fond" (which we are glad to know), but to enable readers to understand "La Connaissance des Tems"; also to prepare them to make observations of the stars, or at least to follow those made by astronomers. We certainly cannot congratulate the author on the knowledge of astronomy shown in his work, of which we will give a few specimens. Speaking of the Moon, he says (p. 57), "la Lune est dans son périgée dans le tems de sa Conjunction et de son Oposition avec le Soleil, et au tems de ses Quadratures elle est dans son apogée."

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[*] This is not in contradiction to the statement that this marking is, in ordinary states of the weather, a really difficult object.

Perhaps we ought not to be surprised that his idea of the magnitudes of the heavenly bodies are *slightly* wild; thus he considers the Sun to be nearly 140 times as large as the Earth and to be equal to 5848 times that of the Moon. Jupiter he takes as 14 times as large as the Earth, their diameters being as 12 to 5; it is not easy to see why he puts the size of Saturn at more then 22 times that of the Earth, as he thinks its diameter is only double. Just now one naturally turns to see what he says about eclipses; one remark is satisfactory:—"ce seroit une foiblesse de croire qu'à cause que le Soleil reste caché un peu de tems, nôtre santé en pût être alterée, et que l'air, l'eau, et les fruits de la campagne en pûssent être gâtés, comme certains Astrologues le font aprehender." The decided way, indeed, in which he rejects judicial astrology (quoting the views of Cicero on the subject) leads one to forgive him a good deal else. He caps the matter by relating some instances in which astrological predictions failed, and one of these it is perhaps worth while to quote:—

"Un Médecin de Spolete, fameux Astrologue, laissa mourir sans remèdes Laurens de Medicis, après avoir trouvé par les astres que sa maladie n'étoit pas mortelle. Mais il porta la peine de sa temérité, car le fils du mort l'aiant trouvé lors qu'il venoit sçavoir l'état du malade, le jetta dans le puits de sa basse-cour. On dit qu'il avoit prédit de soi-même qu'il seroit noié, mais comme on voit que dans peu d'heures deux de ses prédictions, eurent des événemens si diferens, on ne doit ajouter foi ni à l'une ni à l'autre."

We may add that Fizes in his work gives some account of the eclipses of the 14th of May, 1333, the 1st of January, 1386, and the 7th of June, 1415, taken from an old manuscript at Montpellier called "Talamus," to which a reference by Flangergues is quoted in an article on the eclipse of Sept. 7, 1820, in the above volume of the 'Correspondance Astronomique,' p. 403.

Yours faithfully
Blackheath, 1900, May 28. W. T. Lynn.

Eclipses seen from Observatories.

Gentlemen,—

The author of the "Oxford Note-Book" raised the question last month whether there were any previous cases of eclipses being total at an established observatory. I can name a very notable case, viz., the eclipse of 1715, which was observed by Flamsteed at the Royal Observatory, Greenwich, totality lasting $3^m\ 12^s$, and the sky being perfectly clear throughout.

In 1842 Turin Observatory (where Prof. Forbes was stationed) and Vienna lay within the zone of totality, and Berlin did so in 1887, but in this case the Sun was very low. There will be another case next year, when the Royal Alfred Observatory, Mauritius, enjoys $3^m\ 35^s$ totality, the Sun's altitude being 19°.

Algiers enjoyed totality in 1860 as well as in the present year, but I do not think there was an observatory there at that date. The present one certainly did not exist. Yours faithfully,

Blackheath, 1900, June 20. A. C. D. Crommelin.

OBSERVATORIES.

Royal Observatory, Greenwich.—The Astronomer Roya
Report, though made up as usual to May 10, was not present
to the Board of Visitors until June 26, when the Board met
Greenwich under the Presidency of Mr. E. B. Knobel, who t
the Chair in the absence of Lord Lister. The Report is in
usual form, and records, among other things, a large number
observations, more than 10,000 in each element, made with
transit-circle. The reductions of these are all well advan
The changes of level and of reading for nadir-point as foun

different times of the day, which has been remarked on for several years, still continues, and this systematic diurnal movement of the instrument has led to the adoption of a new method of determining azimuth error. Hitherto the practice has been to observe, when practicable, consecutive transits above and below pole of close circumpolar stars, from which the error is deduced; but as this involves the assumption that the azimuth remains constant for twelve hours, this has been given up, and azimuth error is now determined from each circumpolar star by comparing its time of transit with its tabular R.A.

The correction for the R—D discordance for 1899 was $+0''{\cdot}080 + 0''{\cdot}218 \sin$ z.d. The co-latitude of the centre of the transit-circle found in 1899 was $30° \; 21' \; 21''{\cdot}76$, which is $0''{\cdot}14$ less than the adopted value. The correction to the tabular obliquity of the ecliptic was found from observations in 1899 to be $+0''{\cdot}43$, and the mean error of the Moon's place (computed from Hansen's tables with Newcomb's corrections) was $-0^{s}{\cdot}099$ in R.A. and $+0''{\cdot}27$ in N.P.D., deduced from 116 observations.

The new Altazimuth is now in working order. Various repairs and improvements have been made during the year, and determination of the division-errors of the circles is in progress. 25 observations of R.A. and 29 of N.P.D. of the Moon have been made during the year, as well as 1729 observations of R.A. and 1418 of N.P.D. of other objects.

The 28-inch Refractor has been used, as in former years, for micrometrical observations of double stars, of which 492 have been measured. Some time has been spent in examination of Capella, in consequence of Mr. Newall's suggestion that this spectroscopic binary might be seen as a double star. Several observers agreed in detecting an elongation of the image, and the change of position-angle of this elongation was approximately consistent with the suggested period of 104 days.

The Thompson Equatorial has been in constant use throughout the year. An occulting-shutter has been used with the 26-inch refractor to obtain measurable images of such objects as Neptune and its satellite, and of double stars when one of the objects photographed is much brighter than the other objects in the field. Photographs of Polaris and neighbouring stars have been taken for parallax. The 30-inch reflector on the same mounting has been used for taking photographs of nebulæ.

The Astrographic work is proceeding rapidly. The number of quarter-plates completely measured in both positions during the year was 556. The measures are now practically completed between declinations $+64°$ and $+73°$, which covers more than half of the zone allotted to Greenwich. These measures are being printed, and a considerable portion of them is passed for press. The number of stars on the Chart-plates is being counted, and a comparison is given between these numbers and those on the corresponding catalogue plates. For the zone between declination $+65°$ and $+66°$ the numbers are as below :—

No. of Stars with exposure			Number in B.D.	Number in B.D. down to 9·0 mag.
40^{m}.	6^{m}.	20^{s}.		
38,600	7130	1405	1992	762

The photographic spectroscope mounted on the 30-inch reflector has been brought into use and some trial photographs taken. The sun-spot record has been continued as in former years, about which it is remarked that a steady decline in the daily number and area has been observed in the twelve months covered by the Report, August and September 1899 in particular showing a marked sub-minimum.

Beyond the sending of an expedition from the Observatory to observe the Total Solar Eclipse from Ovar, in Portugal, no extraneous work is mentioned in the Report. The staff remains as in the previous year, the total number of its members, including workmen and messengers, being 53.

STONYHURST.—A very small part of the Report for 1899 recently published by Father Sidgreaves is occupied by astronomical matter, although it is a record of a large amount of work. The drawings of the sun-spots made on 183 days of the year seem to show that the minimum is at hand, for during the first half of the year the mean daily was 0·7 units and during the latter half 0·2 units as compared with 2·5 units for the year 1898. Comparing these with magnetic disturbances, it is considered remarkable that of the seven days of the year noted as days of greater magnetic disturbance, four occurred in the two periods of greatest spot-areas of the year, viz. in the middle of March and at the end of June. Observations continue to be made of the solar spectrum, and the stellar spectrograph has been used whenever possible to continue the series of photographs to investigate possible changes in the spectra of selected stars. The work of measuring and mapping these has been interrupted by the sun-spot research, a preliminary result of which was recently presented to the Royal Astronomical Society by Father Cortie.

The meteorological and magnetic observations are given in the usual form.

PUBLICATIONS.

STELLAR PARALLAX [*].—It has been remarked that one tendency of modern astronomy is to systematize research so as to obtain results by wholesale. A proposal of this kind, to determine stellar parallaxes, first suggested as long ago as 1889 by Prof. Kapteyn, has recently found fruition in a volume (No. 1) of the 'Publications of the Astronomical Laboratory of Groningen,' which contains the parallax of 248 stars of the region BD +35°·4013.

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[*] 'Publications of the Astronomical Laboratory at Groningen.' Edited by Prof. J. C. Kapteyn, Director of the Laboratory. Groningen, 1900.

The determination of parallax by photography is not a new idea, the method generally adopted hitherto being analogous to that used when micrometer measures are made for the same purpose, which requires photographs of the region containing the star under discussion to be taken at suitable epochs, and on which the distances of the star from the neighbouring stars are measured, and the variation of these distances gives the parallax. One advantage of photography lies in the fact that many stars can be conveniently used as comparison stars. Prof. Kapteyn makes the most of this advantage, and, in addition, adopts a system rather different in principle.

The first point of difference from previous methods is that the photographs at the different epochs are taken on the same plate. The plate is exposed to the region at the epoch when parallax has its maximum effect. Six months later, when the effect is again a maximum but with opposite sign, it is again exposed, twice at this epoch, and then, six months later, another exposure is given. Needless to say, the plate is so disposed that the four images of any one star do not coincide, but arrangement is made that they lie in a line, and, further, the exposures at each epoch are duplicated or multiplied to increase the accuracy of the measurement; but this point need not be further considered here. Calling the images of any one star A, B, C, D, and assuming that the interval of time between A and B was exactly equal to that between C and D, if the star *had* a proper motion the length AB would be just equal to CD; if, on the other hand, the star has a sensible parallax, AB would differ from CD; therefore if the difference of these lengths be measured, this difference is not affected by proper motion, but is available for determination of the parallax. It is obvious that the simple case considered above, when the times between the exposures are equal, does not always occur in practice, but if t_1, t_2 be the lengths of the intervals, and p and q the lengths AB, CD, the quantity

$$\nu = \frac{t_2}{t_1}p - q$$

must be formed instead of the simple difference.

For any star $\nu = \text{constant} + H\pi +$ terms due to differential refraction, aberration, and instrumental errors, where π is the parallax of the star relatively to the mean of all the stars on the plate. The terms mentioned on the right-hand side of the equation consist of unknown quantities multiplied by factors known for each star from its position on the plate, and it is always possible to choose the epochs of exposure and the direction of the line on which the projected distances between the images are measured, so that H, the parallactic factor, is nearly *four*. When these equations are formed, the constant and the unknowns can be found by their solution; and if the solution be effected by least squares,

$$H \,.\, \Sigma\pi = 0,$$

as was to be expected, since π is the parallax of a star referred to the mean of all the stars on the plate; in fact the terms $H\pi$ for individual stars come out as residuals of the solution.

The method is beautifully simple in its general plan; but, as may be supposed, there are points of detail to be considered. Prof. Kapteyn discusses various causes of error which are worthy of consideration by those who take star-photographs for measurement. There may be errors due to bad guiding or to atmospheric dispersion which would affect the images, and therefore the measures, of bright stars differently to the fainter ones. If all stars were affected equally the results would not be affected, since they are relative to the mean of the plate. A case is pointed out in the volume where the resulting parallax of the bright stars appears to be influenced by some such cause.

The actual results described in the volume are as follows:—Four plates were taken (besides others, the reduction of which is not given) of the same field by Prof. Donner, of Helsingfors, each with exposures according to the scheme above described. Three of these plates were measured, and the relative parallax of 248 stars found on them were deduced. There are no strikingly large values among them, the largest of the parallaxes being $+0''\cdot18$ and the smallest $-0''\cdot13$; but the accordance of the different values for the individual stars as found on the three plates gives considerable confidence in the reality of the values, and fair promise for the success of such a scheme as Prof. Kapteyn proposes. His proposal is, in effect, that when the Astrographic Chart is done, a Parallax Durchmusterung should be undertaken. Comparing the labour involved in the two works, he thinks that the photography of the Parallax Durchmusterung would only slightly exceed that necessary for the Astrographic *Chart* Plates, that the measurement will be comparable with that of the Astrographic Catalogue Plates, and that their reduction would be considerably less laborious.

PRIMITIVE CONSTELLATIONS OF THE GREEKS *.—Assuming it to be worth while to accumulate proof in support of a well-nigh undisputed fact, Mr. Brown in the present volume completes the chain of evidence from the archæological point of view as to the date and place of origin of the older constellation-forms.

To the astronomer the established fact of precession is convincing proof; but Mr. Brown is not concerned with him so much as with the student of cuneiform literature, which can hardly be included in the category of the exact sciences: so it need cause no surprise to find that some of his arguments tend to establish nothing more than a rather strong presumption of the correctness of his theory, while in more than one case he admittedly leaves a point open to question, in such a manner as to beget more confidence in his conclusions where he definitely reaches them.

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* 'Researches into the Origin of the Primitive Constellations of the Greeks, Phœnicians, and Babylonians,' by Robert Brown, Jun. Vol. II. Williams and Norgate, 1900.

It is partly for this reason, in addition to the marked attention given to the conjectures of other eminent orientalists (such as Professor Sayce, Professor Hommel, and Mr. Pinches), even where they are at variance with his own, and to the greater variety and more general interest of this portion of the work, that this volume is far more readable than its predecessor.

The author makes a very ingenious and fairly convincing effort to reproduce in its entirety a complete Euphratean planisphere from three very small fragments in the British Museum collection. A later chapter is devoted to a careful examination of the Tablet of the Thirty Stars, which are assumed to be those referred to by Diodorus, and then follows a *résumé* of Euphratean astronomical knowledge, with a valuable list of identifications arranged conveniently in parallel columns.

There would appear from some instances (*e. g.*, for one ideograph, the three distinguished scholars above referred to give respectively, Bildara, Nidar, and Issi) to be a fascination in the study of cuneiform akin to that of the solution of the picture puzzles of certain popular weeklies, with the doubtful advantage of the total absence of "official" solutions.

Mr. Brown's work is not free from the influence of the solar myth, though in a less degree than the first volume. And on this subject it may be mentioned that since Sun and Moon are only mutually exclusive when the Moon is full, and since the typical and conventional Moon is nearer new than full, it is by no means an *axiom* that the representation of Gemini, head to head, necessarily implies that the original twins were the Sun and Moon, though it is quite possible, and even probable.

It would be unjust to hold Mr. Brown responsible for the weird spelling of names, which is apparently governed by a convention adopted by cuneiform scholars, but it may fairly be suggested that inasmuch as language spoken and the same thing written are known to differ greatly (*e. g.*, Chinese), there is at least a possibility that the contemporary Greek rendering of foreign names was not so phonetically incorrect as is generally assumed by Orientalists, and in any case such combinations of English letters as *khsh* or *qqa* are "Caviare to the general."

The volume is adorned with two carefully drawn plates and many illustrations from Euphratean art. W. W. B.

NOTES.

Comet Notes.—Giacobini's Comet (a 1900) has been observed on its emergence from the Sun's rays, its place agreeing closely with Berberich's ephemeris for Berlin midnight, which is continued below:—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
June 28....	23 56 17	40 31	Aug. 3....	19 22 21	39 50
July 2..	23 36 32	42 31	7....	18 57 55	36 39
6...	23 12 47	44 21	11....	18 37 45	33 21
10....	22 44 43	45 51	15....	18 21 18	30 7
14....	22 12 29	46 51	19....	18 8 0	27 1
18....	21 37 4	47 7	23....	17 57 19	24 9
22....	21 0 16	46 31	27....	17 48 46	21 30
26....	20 24 19	45 0	31....	17 41 57	19 5
30....	19 51 16	42 42	Sept. 4....	17 36 41	16 55

The comet is of about the 10th magnitude, and will slowly brighten till the middle of July, after which it will fade pretty rapidly.

A. C. D. C.

Minor Planet Notes.—In addition to the Arequipa observation, which was referred to last month, the planet Eros was observed by Prof. Howe, of Denver, about four hours before the recent total solar eclipse. Some papers announced that the observation was made during totality, but this was an error.

Prof. Howe's observation indicates that the ephemeris computed from Millosevich's elements requires the following corrections:—R.A. $+4^{\mathrm{s}}\cdot14$, Dec. $+33''\cdot0$.

It would appear from these that Millosevich's value of the mean daily motion is too small by about $0''\cdot1$.

Two new planets have been discovered, both by Wolf and Schwassmann at Heidelberg, viz., FG on May 22, and FH on June 4.

A. C. D. C.

Double Stars.—In Nos. 3645–46 of the *Astronomische Nachrichten* Prof. S. J. Brown gives measures of double stars made in the years 1897–8–9 with the 26-inch Washington refractor. These really form an extension of the fine series made by Prof. Asaph Hall, and we congratulate Prof. Brown on this his first instalment. The weather was disappointing, and consequently the number of difficult pairs falls short of what he had hoped to secure. There are about 250 pairs, of which 112 are under $1''\cdot0$ apart.

Prof. Knorre continues his observations at Berlin. He uses a double-image micrometer on a 9-inch equatorial, and evidently finds that he can do the best work on stars about $3''$ separation. He gives results of 27 pairs in *Ast. Nach.* No. 3632.

Prof. E. E. Barnard has made some very fine measures for parallax of the star DM $+37^{\circ}\cdot 4131$, which he publishes in No. 482 of the *Astronomical Journal*. He is of opinion that this star has no sensible parallax.

While making the parallax measures he found that the star DM $+37^{\circ}\cdot 4133$ was a triple:

1899·82 ……	250·4	0″·92	9·5	9·8 mag.
1899·82 ……	111·7	7 ·87	..	15 „

In the same number he gives recent measures of Procyon made with the 40-inch Yerkes refractor:

1899·85 ……	343·2	59″·28	4 nights.
1900·06 ……	336·0	5 ·09	6 „

PHOTOGRAPHIC OBSERVATIONS OF NEPTUNE.—M. Kotinsky has photographed Neptune and satellite with the 13-inch photographic telescope at Poulkova, and publishes his measures for nine nights in No. 3642 of the *Ast. Nach.* They agree with the Greenwich measures in suggesting slight corrections to ephemeris.

THE MOON HOAX.—Mr. S. A. Saunder writes to us with reference to the note under this heading in last month's number, in which it appears that we did considerable injustice to Arago. Mr. Saunder, writing from recollection, as it is some years since he read the history of the matter, says that when the popular excitement was at its height, Arago, who was then President, read extracts from the recently published "Moon Hoax" before the Academy of Sciences, and said that he thought the least that men of science in Europe could do was to prevent Sir John Herschel's name from being associated with such nonsense. He took the matter very seriously, and with tears in his eyes besought the Academy to formally express its opinion to that effect, the result being that amid roars of laughter the account was formally declared to be incredible.

FLAMSTEED'S 'HISTORIA CŒLESTIS.'—We are indebted to Mr. Woodd Smith for the following interesting addition to astronomical history, which has been extracted from the Lord Chamberlain's Warrants (George I.):—

Copys of Astromicall Observacons to be deld to Mr. fflamsteed.

His Majesty being informed that there are now remaining in the hands of Mr. Annsham Churchill printer three hundred and forty copys of y^{e} Astronomicall Observacons made by the Rever^{d} Jno Flamsteed his Maj^{ts} Astronomer at the Observatory in Greenwich comprizd in a book entitled Historia Cœlestis wh was printed at the Expens and charg^{e} of his late Royall highness Prince George of Denmark These are to Signify his Maj^{ts} pleasure that you give orders for delivering to the s^{d} Mr. Jno Flamsteed three hundred

Copys of the s^{d} book entitled Historia Cœlestis as a present from his Maj^{ty} and that you likewise give directions that the remaining forty copys be sent to me to be disposed of as his Majesty shall think fit together wth an acc^{t} of how much money has been received and expended in that service. And this shall be your Warrant Given und^{r} my hand this 30th day of November 1715 in the second year of his Majs Reign.

To S^{r} Isaack Newton Knt.
the hon^{ble} ffrancis Roberts Esq.
S^{r} Christop Wrenn and Dr.
Arbuthnot referrees to his Roy^{ll}
highness Prince George of Denmark.

Bolton.

Mr. Rowley for a Double Telescope for His Ma^{ty} £17.

These are &c. to Mr. Jno Rowley the Sum of Seventeen Pounds for a Double Telescope with Glasses placed in Silver &c. for His Majesty. And &c. Given this 2nd day of May 1724 in the Tenth Year of his Majesty's Reign.

To Charles Stanhope Esq. T. Coke.

Mr. Wright Mathematical Instrum. Maker &c.

These are &c. to Mr. Thomas Wright Mathematical Instrument Maker to His Majesty the sum of Three hundred Thirty six pounds for making new all the Machinery and wheel work to perform the Motions of the planets &c. to the Great Orrery in her Majesty's Gallery at Kensington. Also to pay him the sum of fforty five pounds three shillings for office ffees amounting in all to the sum of Three hundred Eighty one pounds three shillings. And &c. Given &c. this 21st Day of December 1733. In the Seventh Year of His Majesty's Reign.

To the R^{t} Honble y^{e} Lord Hobart. Grafton.

A proposed Memorial to the late Mr. G. J. Symons.—At the invitation of the President of the Royal Meteorological Society, a meeting was held at the rooms of the Society on Thursday afternoon, May 31, to consider the question of a Memorial to the late Mr. G. J. Symons, F.R.S., the distinguished meteorologist and founder of the British Rainfall Organization. It was resolved unanimously that the Memorial should take the form of a Gold Medal, to be awarded from time to time by the Council of the Royal Meteorological Society for distinguished work in connection with meteorological science. The meeting appointed an executive committee, representing many of the Societies with which Mr. Symons was associated, to take the necessary steps to raise a fund for that purpose.

Contributions will be received by the Assistant-Secretary, Mr. W. Marriott, 70 Victoria Street, Westminster.

Recent Sun-spots.—The expiring sun-spot period is dying hard. There have recently been two spots which may be considered remarkable considering the stage of the cycle. A large double spot passed the central meridian about June 19 in latitude south of but near the solar equator. A second spot, first seen at Greenwich on June 23, paſsed the central meridian late on June 25.

and increased in size until, on June 29, it appeared to be as large as 500 millionths of the visible hemisphere. The heliographic latitude of this is about the same as that of its forerunner.

The first of these sun-spots has figured to some extent in the daily press. As is usually the case when a sun-spot occurs in the summer months, some one enunciated the opinion that this heralded very hot weather; but another journal made the remarkable statement that this spot was discovered by the large Paris-Exhibition telescope, and that persons with *ordinary telescopes would be able to detect this spot in about a fortnight.*

An Annual Catalogue of Astronomy.—Dr. Wislicenus' book, mentioned at the Meeting of the R.A.S. reported in this number, will be found a useful publication. It is a tabulation under subject headings of all the astronomical articles and notes which have appeared during the year 1899, with a little *résumé* of each. It must have involved a large amount of work, and it is to be hoped that it will get enough support to warrant Dr. Wislicenus in continuing the publication in future years.

Interest in double-star work is ably maintained at the Lick Observatory. Recently attention was drawn to the fine lists of new doubles discovered by Profs. Aitken and Hussey, and we now have the pleasure to notice the measures of 204 pairs by Prof. Aitken.

These are published in *Ast. Nach.* 3638–39, and are composed for the most part of very difficult pairs, which have now been measured for the first time since their discovery by Prof. Burnham 1889 to 1891. In some instances rapid motion is indicated.

From an Oxford Note-Book.

The eclipse of May 28 last appears to have been observed successfully everywhere. Eclipse weather is curiously unanimous: it is either fine all along the line or equally cloudy. In 1887 and 1896 it was cloudy; only one party in Japan saved the situation in 1887, and in 1896 the Nova Zembla expedition, organized at the last moment by one of the enterprising and successful Baden-Powell family, triumphed over the numerous expeditions to Norway and Japan. In 1898 and 1900 just the reverse has happened; it was not very surprising that the weather should be uniformly good in India, but when Algeria, Spain, Portugal, and the States have all been equally favoured, there is occasion for hearty and mutual congratulation.

What is the total haul in the numerous nets spread will not be known for some little time. The photographs of the English expeditions made a brave show at the Royal Society Soirée on

June 20, and our American friends will no doubt have a great deal to tell us. Professor Barnard writes delightedly to tell me of the success of the 60-foot lens, pointed to the coelostat. He brought his photographs home to develop, and is developing them with extreme care. "I am going slow," he says; "I don't want to develop in haste and repent at leisure." So he announced on June 8 that No. 1 (the 1^s exposure) had turned out a beauty. On June 12 he writes again:—"I have just developed the second of the seven plates with the large coelostat; it is the 2^s exposure. The negative is very beautiful and clear. The inner corona and the prominences are splendidly shown to the Moon's edge.... The polar fans are very beautiful....On this last plate I used Pyro for developer, and like it best." It will no doubt interest others to hear of this very careful procedure. Of course there are risks in bringing photographs home, as experience has shown, especially when the distance is great; it is a case for balancing advantages and disadvantages. Prof. Barnard has five more plates to develop, $\frac{1}{2}^s$, 8^s, 30^s, 14^s, and 4^s. We shall hope to hear good news of them all, and I am very glad to hear his approval of the coelostat.

OUR Algerian expedition may perhaps be allowed a little glow of satisfaction over one result obtained immediately, which really settles a great deal that was doubtful. It has been stated that even the best photographs of the corona fell sensibly short of the reality; that an artist's eye could see *much* more in the original, especially as regards fine detail near the limb. In these notes for May (p. 227) I referred to this fact, and mentioned that Mr. W. H. Wesley, who has had so much experience in examining coronal photographs to make drawings from them, was going to Algiers to see how far this was true, and that M. Trépied had most kindly put at his disposal the equatorial coudé. The result was a gratifying success. Mr. Wesley was enabled to make the definite announcement that photographs are capable of showing practically everything; which goes a long way towards relieving us from the obligation of making eye-observations at all. He makes this statement with considerable confidence, and this is the more noteworthy in view of the fact that he did not approach the experiment without considerable misgiving. It was a great deal to ask of him, that he should pronounce judgment in so important a matter; and on the day of the eclipse, when he obviously felt the responsibility hang heavy upon him, we felt that perhaps we had asked too much. But fortunately the event relieved us of anxiety. Mr. Wesley found that he had a clear case on which he could give a verdict without hesitation, and I venture to refer to his previous anxiety in order to bring out the value of the statement more clearly. His way of stating the case should be noted: it was not that he did not see a most beautiful sight in the fine instrument lent him; indeed his first impressions were just those which others experience in seeing the corona for the first time—amazement at the beauty of its structure. But after a few seconds' examination he found that it was a familiar sight—he had seen essentially in photographs all that there was, and he states the case accordingly, "*People don't know what there is in the photographs.*" I think these words may fairly be put in italics.

LOOKING back at last month's notes it is odd to read that we were, at the time of writing (eight days before the eclipse), the only strangers who had arrived at the Algiers Observatory. Visitors came in considerable numbers afterwards, and stuck up all sorts of instruments. Poor M. Trépied, anxious as he was to do all he could for others, must have had his patience sorely tried by some of the demands made upon him at the last moment. One gentleman, whose instruments were delayed in transit by some accident, arrived on the evening before the eclipse with his apparatus in two cabs, and proceeded to spend the night in erecting the polar axis on a wooden pier. He had just arrived when we were leaving for dinner. When we came back to put in our plates the wooden pier was being levelled and adjusted. There was an Arab to dig holes for the legs, a Frenchman to interpret and assist generally, and the proprietor was a German. When any operation was being conducted the three voices in consultation rose crescendo until the voice in command clinched matters with a stentorian *sapperrrlot*! and then there was a dead silence for the space of five seconds. But the adjustment proceeded. When we left the observatory at 2 A.M., the polar axis was nearly completed, and the next morning the whole instrument was erected (including the table for the beer), notwithstanding a visit of the whole party to our hotel for refreshments, which aroused every tired sleeper in the place. There was much to admire in the energy of this party, but we could have wished that they had been more careful not to let it overflow on several occasions.

A DELIGHTFUL experience was the advent of MM. Tacchini and Ricco. They would not stay with us, preferring seclusion a little further along the line, and, as the event proved, they were wise. But our meeting with them under M. Trépied's hospitable roof was an occasion to be remembered. M. Tacchini was in all respects younger than when we were in the West Indies together in 1886, and was the life and soul of the party. We recalled with interest the incidents of that eclipse—the tropical rains and prickly heat, the fever and mosquitoes, the ewe-necked stumbling horses that had to be ridden, and M. Tacchini's magnificent jump on to the quay when the lumpy sea made the usual method of leaving the steamer uncertain. We remembered, too, one of the secondary actions of the eclipse in bringing wealth to a certain French-speaking negro. This scare-crow, originally with no clothing to speak of, assisted M. Tacchini for about eighteenpence a day, which remuneration favourably affected, first of all, his physical appearance, then his dress, which improved by leaps and bounds, and finally, on eclipse day, the well-fed, well-clothed gentleman of colour galloped up to undertake his duties on a horse!

ECLIPSE day with us in Algiers was the finest of our stay. The Sun rose and remained in a sky nearly as cloudless as that of India, and ultimately set in the sea showing a beautifully clear "green ray." We were busy up till the last moment—indeed we had really too little time for our preparations, though we worked very hard all the time, and we envied those astronomers who strolled up to look at the eclipse with a telescope. Half a dozen English visitors who had kindly offered to assist us, and had rehearsed the programme, were quite useful in protecting us from enquiring crowds of visitors. We did a little rehearsing in dumb show, and then all was ready, but not till some time after first contact. The observatory grounds presented an interesting appearance by this time. The last serious observer had arrived and pinned his telescope to a telegraph post, with his wife to record at a small table near. An enterprising photographer from Algiers was very busy. The beer was put out on the German table aforesaid—but I would not have alluded to this little peculiarity if the party had only had the decency to keep quiet at the critical time. As totality came on there arose from this particular party an excited babble which made them a nuisance to everybody, and quite spoiled one or two important observations. Otherwise all went well, and we owe a good deal to those who so ably assisted us.

MY account of this expedition is, I fear, very meagre; but the fact is we were, as above remarked, exceptionally busy. Our instruments arrived late and cut down our precious time to the minimum, for we had a rather extensive programme. Consequently it was, as one of us remarked, "a (very) serious eclipse," and there is not so much of incident to record as usual. There *might* have been. Algiers is a most delightful place, and the little village of Bouzareah, where we stayed, has the advantage that the mosquitoes are less poisonous than those in the city, and the wine of the village is better. We could have spent interpolated days of holiday with the greatest comfort and profit, and cherish hopes of going back at some future time to recover those fairly due to us. But we did not manage to get them during our busy time of preparation nor in the week of developing plates and packing up, and so there is less to report here than usual. If we go back and get those days I will try and make up the deficiency.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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THE BRITISH ASTRONOMICAL ASSOCIATION.

The concluding Meeting of the Session of the British Astronomical Association was held at Sion College on Wednesday, June 27, *Mr. W. H. Maw*, President, in the Chair.

The names of nine Candidates for Membership were read and suspended, and the election of three new Members was confirmed.

The President said the Members present would be glad to know that they had with them that evening Mr. Ambrose Swasey. Mr. Swasey had been paying a visit to Paris, and had kindly promised to give the Meeting a few facts relating to the great telescope of the Paris Exhibition, which he had had an exceptional opportunity of examining.

Mr. Swasey said he was much gratified to have the pleasure of meeting with his fellow Members of the Society. While in Paris he had, in company with Members of the Jury of Awards, visited the great telescope, which was fully explained to them by M. Gautier, the constructor. From an engineering and mechanical standpoint, the instrument appeared to have been admirably designed and of excellent workmanship. The great reflecting mirror was so well mounted and so perfectly balanced, and the friction of the sliding and revolving parts so reduced to a minimum, that it could be moved to any position with the greatest ease. All the quick and slow motions, together with the reading of the fine circles, were so arranged as to be readily accessible to the assistants. As to the optical parts, the mirror and the photographic lenses were then in place, but it was understood that they were not yet fully perfected owing to lack of time. The disks for the photographic and visual objectives were made by M. Mantois, and although the disks for the latter have not yet been figured they were said to be of most excellent quality.

The President said the chief business of the evening was to receive the reports of the eclipse observers, but before any of the papers were read or any observations reported, he would call upon Mr. Maunder, who had a vote of thanks to propose to the authorities in the various countries who had aided so much in making this series of observations a success.

Mr. Maunder proposed the following resolution :—"That this Meeting of the British Astronomical Association desires to record its sincere appreciation of the ready courtesy and valuable assistance rendered to the members of the various expeditions in the observation of the total solar eclipse of 1900 May 28, in the United States, Portugal, Spain, and Algiers."

Dr. Downing said he seconded the motion with very great pleasure. He really found it difficult to speak in sufficiently strong terms of the courtesy and of the hospitality with which the members of his party were received in Spain.

Mr. Crommelin said he could re-echo, with regard to Algeria, what had been said by Dr. Downing as to the kindness with which the Spanish expedition had been treated.

Mr. G. F. Chambers said the remarks that had been made by Dr. Downing and Mr. Crommelin applied exactly to the circumstances of Portugal, yet, if anything, they understated the kindness shown and facilities offered to the British astronomers who went to that country.

The resolution was carried unanimously.

Several Members then spoke as to the observations made by them of the recent solar eclipse.

Dr. Downing said his party took up their position at Plasencia, about 140 miles south-west of Madrid. He used an opera-glass with spectroscope fitted by Mr. Thorp. This acted most satisfactorily, surpassing his expectations. Mr. Thorp had fitted a grating to one of the object-glasses of the binocular, by which the spectrum could be observed, while through the other tube of the binocular the corona could be observed directly. His object was to observe the distribution of coronium with the spectroscopic arrangement, and to compare it with the extensions of the corona viewed directly through the other tube of the binocular. He observed that the coronium arc was much broader and more diffused than the neighbouring arcs of magnesium and helium which were visible in the spectroscope at the same time. He estimated the average breadth of the coronium arc to be about one-eighth of the diameter, but at a special part it was very much wider, being about one-fifth of the diameter approximately. This part corresponded to the position-angle of about 270°, and that would tend to show that the additional amount of coronium was present in the corona near the base of the large extension on the western limb of the Sun. According to his observation, therefore, the general height of coronium in the corona on this occasion was a little over 100,000 miles, but at this special part, near the base of this branch of the corona, it extended to about 180,000 miles.

Mr. Maunder said the party at Algiers was a fairly large one, and they made their observations from the roof of the Hôtel de la Régence. His wife used a telescope, kindly lent by Mr. Coleman, on which were mounted two cameras, each with a rapid rectilinear lens of focal length 18 inches, which they used with a stop of 1½ inch. The plates were exposed 48 seconds—practically the whole of totality—in both cameras. He personally used the same mounting as in India, with a pair of Dallmeyer stigmatic lenses on it, and exposed them for 48 seconds. Miss Irene Maunder took a series of six photographs of the corona. A remarkable feature of the photographs—a feature which he had not clearly seen on any photograph of a previous eclipse—was the presence of dark rays in the corona, not mere rifts between two arms of the corona, but dark lines which seemed to stretch out on the background of the sky itself. Mercury, being very near the Sun and very bright, had come out in almost every photograph of the eclipse. Mr. Maunder said this was the third eclipse he had seen, and it struck him that it was the most beautiful of the three. The corona of 1886 and that of 1898 seemed to be white, of the character of the whiteness of the electric arc. This one seemed to be of a warmer tint, more like that of cream or ivory.

Mr. Crommelin said that he used a tele-photo lens kindly lent by Mr. Cavan, but the photograph taken with this did not come up to his expectations, he thought owing to spherical aberration in the lens. He also used a Dollond 3-inch telescope, with which he observed the partial eclipse by projection, and during the first 50 seconds of totality studied the inner corona visually. He observed Baily's beads beautifully by projection. They began to appear 20 seconds before totality, and when he gave the 10 seconds signal the whole crescent was broken up into beads. The edges of the beads looked quite straight, perpendicular to the Moon's limb, not like serrated edges of mountains or anything of that sort, but absolutely straight cuts.

Mr. C. L. Brook gave an account of the shadow-band observations which he made at Algiers. He thought the word "bands" was a misnomer, and that shadow "ripples" would be a more correct term to employ. He said the "bands" appeared 3¼ minutes before totality, travelling from about 30° W. of N. to 30° E. of S. He estimated their speed at 1½ yards in a second. They were very faint, and after totality grew fainter and more irregular.

Mr. Evershed stated that he had selected Mazafram as his station, believing it to be 1¾ miles inside the limit of totality. Unfortunately it proved to be just outside the limit. On examining the series of photographs obtained, however, he found that four of them gave excellent images of the flash spectrum.

The Rev. C. D. P. Davies said the corona was the most magnificent sight he had ever witnessed. It appeared to him as a forest of streamers, beautifully defined, with not the slightest approach to a halo or haze around them.

Mr. Keatley Moore said that he and Capt. Carpenter drew the corona as seen by the naked eye, each taking half the corona.

Mr. C. T. Whitmell, who observed at Navalmoral, said that, though specially looked for, the sweep of the shadow was not observed either in the air or on the ground. No shadow-bands were seen either before or after totality. The brightness during totality certainly exceeded that of the full Moon, but was quite unlike moonlight, resembling more a late twilight.

A large number of photographs of the eclipse and of the eclipse parties and stations were thrown on the screen during the evening.

The Meeting adjourned at $7^h\ 20^m$ P.M.

The Astrographic Chart Conference.

THE fourth Meeting[*] of the International Permanent Committee opened at Paris on Thursday, July 19. There were present, as Members of the Committee:—Anguiano[†], *Tacubaya*; Baillaud, *Toulouse*; Bakhuyzen, *Leiden*; Christie, *Greenwich*; Dunér, *Upsala*; Gill, *Cape of Good Hope*; Paul Henry, Prosper Henry, Loewy, *Paris*; Rayet, *Bordeaux*; Ricco, *Catania*; Scheiner, *Potsdam*; Tacchini, *Rome*; Thome, *Cordoba*; Trépied, *Algiers*; Turner, *Oxford*; Valle, *Tacubaya*; Viniegra, *San Fernando*; Weiss, *Vienna*. The following were invited to take part in the proceedings:—Bassot, Bigourdan, Prince Roland Bonaparte, Bouquet de la Grye, Callandreau, Elkin, Eginitis, Gaillot, Gautier, Hagen, Hartwig, Mdlle. Klumpke, Laussedat, Legrand, Leveau, Lippman, Lumiere, Newall, Perrotin, Poincaré, Rees, Stephan, Verschaffel, Wolf.

After a gracious address of welcome by M. Loewy, Officers were elected for the Meeting as follows:—*President*: M. Loewy. *Vice-Presidents*: Sir David Gill, Prof. Bakhuyzen. *Secretaries*: MM. Trépied and Dunér (the latter in place of Prof. Donner, who was absent through ill-health).

Letters of regret for non-attendance were read from Profs. Backlund, Donner, Kapteyn, O. Struve, L. de Ball, Capt. Rodrigues, Messrs. Baracchi, Russell, and Brown (Washington).

Letters were read from Dr. Becker, Dr. Bruns, Profs. Hale, Keeler, Leavenworth, and Pickering, who all promised to help in making observations of Eros during the coming opposition. The following members were elected as a Committee to draw up a scheme for determination of the Solar parallax from observations of Eros:—André, Bakhuyzen, Christie, Elkin, Gill, Hartwig, Prosper Henry, Trépied, Weiss.

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[*] The Meeting to initiate the work was in 1887. Previous Meetings of the Committee have taken place in 1889, 1891, 1896.

[†] M. Anguiano has resigned the Directorship of the Tacubaya Observatory, having been appointed Superintendent of the Mexican Geodetic Commission. He has been succeeded by M. Valle.

Agenda.

1. To receive Reports from Directors of Observatories upon the state of the work.
2. To discuss the action to be taken in view of the fact that the work of the Astrographic Chart is not proceeding at the Observatories of Rio Janeiro, La Plata, and Santiago (Chili).
3. To consider whether any help in the way of measurement or reduction of plates should be given to those Observatories which have not sufficient funds to undertake the work.
4. To receive reports upon the different methods used for determining photographic magnitudes.
5. To consider questions relating to the publication of the Astrographic Catalogue:
   (*a*) In the case when rectangular coordinates are published, what elements should be supplied in addition?
   (*b*) The arrangement, typographical and otherwise, of the printed catalogues already begun.
   (*c*) The probable number of stars in the different zones.
6. To discuss questions relative to the "étoiles de repère":
   (*a*) Is it necessary to have meridian observations of these stars made at an epoch not far from that of the exposure of the plates?
   (*b*) Choice of the fundamental catalogue which will connect these stars.
7. To consider the overlapping of a zone with one adjacent, which is assigned to another Observatory. Shall each of the Observatories reduce these independently?
8. To consider whether Resolution (8) of 1896 should not be modified *.
9. To consider what precautions to take to ensure the most absolute fidelity in the reproduction of plates by heliogravure or by any other process.
10. To consider the steps to be taken for the preservation of the plates.

To organize a research for the determination of the solar parallax by means of photographic observations of the planet Eros at the coming opposition.

The reception of the reports summarized in the following table completed the proceedings of the first Meeting:—

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* Resolution (8) of 1896 was as follows:—"For the construction of the Chart the second series of plates, namely those of which the declinations of the centres are odd numbers, shall be taken with three exposures of 30 minutes each. This time of exposure will of course be reduced should photographic plates of greater sensitiveness be produced."

Observatory.	Number of plates in Zone.	Catalogue Plates.		Chart Plates.	
		Taken.	Measured.	With triple exposure.	With one exposure.
Greenwich	1149	1106	608	none	1076
Rome	1140	476	15	106	
Catania	1008	1008	36		
Helsingfors	1008	1008	380	(200)	(200)
Potsdam	1232	1232	(400)	(77)	(
Oxford	1180	736	736	none	none
Paris	1260	1260	650	97	
Bordeaux	1260	402	293	17	
Toulouse	1080	540	135	45	
Algiers	1260	1260	497	56	
San Fernando	1260	(1260)	145	none	none
Tacubaya	1260	746	203	none	none
Cape of Good Hope	1512	1512	106	(378)	(756)
Sydney	1400	1400			
Melbourne	1149	900	...	...	(575)

The numbers in brackets are given with reservation, as they are based on approximate statements. At Catania and Sydney the Chart plates are being taken, but the number is not known. The numbers given for the Chart plates taken at the French observatories —Paris, Bordeaux, Toulouse, Algiers—are those of the number of fields of which reproductions by heliogravure have been made. Besides these, other negatives have been taken.

At Greenwich, Rome, Oxford, San Fernando, and Tacubaya the plates are being reduced by the linear method, which has been described in these pages. At Helsingfors, Bordeaux, and Algiers the reductions are being effected by methods designed by the Directors of the respective observatories. At Paris the method due to the brothers Henry is in use. At Potsdam an approximate reduction only is being made; and at the Cape and Catania the reduction has not been begun. At Melbourne and Sydney the work of measurement is in hand.

M. Trépied computes that at the Algiers Observatory the cost of measuring each star without any reduction is $0^{f}{\cdot}20$.

At the next meeting the second item of the agenda was partially settled in a satisfactory manner by the offer of Dr. Thome, of Cordoba, to undertake the zone -24° to -31°, hitherto assigned to La Plata. It is needless to say that the offer was received with enthusiasm. M. Enrique Legrand, of Monte Video, stated that he had induced his government to found an observatory, and would undertake the zone -17° to -23°, allotted to Santiago. It [illegible] suggested that the remaining zone, -32° to -40°, might possi be done by the new observatory at Perth, West Austr

The discussion of (3) led to the consideration of representatives were almost unanimously of opinion that the measured rectangular coordinates of stars on the Catalogue plates should be printed, and that it would be sufficient to publish the constants of the plates necessary for the determination of measured standard coordinates, together with the R.A. and Declinations of the "étoiles de repère" of the stars used to determine these. But, and this appears to be the main point settled at the Conference, it was not considered necessary that the equatorial coordinates, the R.A., and the Declination of all the stars should be given in the Astrographic Catalogue. In the first place, it would entail a vast amount of computation, which it is not expedient to do yet—first, because the places of the reference-stars, which are in most cases now being observed with meridian instruments, are not known with such accuracy as they will be a few years hence; and, secondly, because it is possible that further investigation will reveal improved methods of treating the plate-constants. It is to be hoped that this opinion was ratified by a formal resolution, for to print now simply the measures and the plate-constants deduced from these, as is being done at Greenwich and Paris, seems to be the most practical course. It does not entail a great expense, which is an important point, for possibly Greenwich and the Cape are the only observatories whose resources at present could stand the strain, as Paris is now spending a great deal on heliogravure; but to print the measures is probably within the means of all the co-operating observatories, so that all can so far work on identical lines, and it is open to any or all to convert these into equatorial coordinates in the future if it is thought well to do so. The sole point that can be mentioned in opposition to this course is that Potsdam has begun the publication of the Catalogue, giving the approximate R.A. and Declinations of the stars. If Dr. Scheiner can see his way to come into agreement with other observatories, the Catalogue will be set going on homogeneous lines, and this will be not the least of the happy results of the present Conference.

At the next sitting discussion took place on the methods adopted for determination of photographic magnitudes. Several persons described their methods of measuring diameters, and it quickly appeared that in this particular the *scale*, as a measuring instrument, has an advantage over the screw. There are some cases in which a mental impression is a surer guide to the truth than accurate measurement, and the estimation of diameters by means of a scale seems to be a happy combination of both methods. It not only gives accordant results, but for rapidity of work leaves nothing to be desired. The Conference passed no definite resolution on the point, but left each observatory to follow its own devices.

With reference to the number of stars, mentioned in 5 (*c*), accurate figures were not given by all the representatives, and in making estimation many counted the stars on the whole area of the plates, in fact each star was counted as many times as it occurred in both series of odd and even declination. On this basis the average number to be dealt with by each observatory is 400,000.

The remaining time of the Conference was occupied with subjects relating to the Chart proper. In 1896 it was resolved the series of Chart plates whose centres have declination of an odd number of degrees should be taken with three exposures of 30 minutes each, the plate being slightly displaced so as to give three images very close together. The French observatories have been taking plates on this plan, as shown in the tabulated report above. At Greenwich all Chart plates have been taken with one exposure of 40 minutes. It has now been resolved that each observatory may do as its Director pleases—that is, all the plates taken by that observatory may have one or three exposures.

In reproducing the Chart plates the French observatories have been proceeding in the most generous way. The enlarged copies of the negatives made by heliogravure, specimens of which have been sent to the Royal Astronomical Society, are beautiful but expensive. It appears that to make such as these would cost each observatory about £10,000, and it is doubtful whether every government concerned would be willing to vote this. For the moment it is resolved that copies of the Chart plates shall be made, but the actual method for doing this is not defined.

Representative Stellar Spectra *.

THE series of the Tulse Hill Publications opens with *éclat*. This is as it should be. So remarkable an establishment as Sir William Huggins's observatory deserves to be distinctively represented in the literature of astronomy, and we can bestow no higher praise upon the volume before us than to say that it is worthy of its origin. It professes to be an 'Atlas.' The plates are then of primary importance in the design of the authors, and we doubt whether so fine a collection of spectrographic reproductions is elsewhere to be found. Since in the apparatus employed for exposing the negatives glass is everywhere replaced by quartz and spar, they extend to the furthest accessible region of the ultra-violet, and they include within their lower limit the green line of hydrogen (H β). An unusually long range of analysed light is thus covered by these wonderful pictures, which are not only masterpieces of astrophysical art, but possess an evidential value due to special selection and arrangement. The star-spectra they depict are not shown as proofs of manipulative skill, or for their intrinsic beauty and interest, but with a view to their interpretation. What do they mean, absolutely and relatively? is the fundamental question raised by the entire work. And it is sought to be answered in a weighty discussion on the "Evolutional Order of the Stars," which occupies a prominent position in the text.

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* 'An Atlas of Representative Stellar Spectra.' By Sir William Huggins, K.C.B., and Lady Huggins. William Wesley & Son, 1899.

The authors do not offer a system of classification rounded off to completeness in every detail. Their object was to lay down certain broad lines of sidereal development, reserving for future discussion the status of such exceptional objects as Wolf-Rayet stars, carbon-stars, and bright-line helium stars. To correlate types of spectrum with stages of growth—to trace the changes in absorptive quality induced by the slow dissipation of radiative energy—this was the task, and a sufficiently formidable one, to which they addressed themselves.

They have performed it with a large measure of success. The order of evolution chosen by them seems to be prescribed by the irresistible logic of facts. Assuming—as we are fully entitled to do—that gaseous nebulæ condense into stars, there is really no choice but to place those of the helium variety at the start of the procession. And helium stars pass with unbroken continuity, through Sirian and solar, to stars with fluted spectra. The gradations by which these last sink towards extinction are not specified, and can barely be conjectured. Perhaps the light-vicissitudes characteristic of third-type objects may be a symptom of decay; but in our present ignorance as to the causes of stellar variability, it would be rash to pronounce a decided opinion. All that can be regarded as plausibly made out is, that the "youngest" stars show a helium-spectrum, that, later on, hydrogen predominates over and eventually excludes helium, and is itself, in turn, superseded in importance by a crowd of metallic elements, absorption by which is reinforced, with a still further advance in age, by the superposition of dusky bands, perhaps of composite origin. We are thus, in a manner, *compelled* to assign an age somewhat past maturity to such stars as Betelgeux, Antares, and Mira, notwithstanding the display of bright hydrogen and helium by the unstably shining members of the class. Nor does it appear feasible to set carbon-stars far apart from them on the time-scale. The degree of condensation reached by ruddy stars distinguished by eight or nine bands sharp towards the blue, can scarcely be very different from that attained by crimson stars badged by five or six bands sharp to the red. Nevertheless it is impossible to assert that there is any true genetic tie between them, or even that they are of collateral descent. The pedigree in fact of fourth-type stars remains obscure, while the chemical meaning of their spectra is, to a considerable extent, legible.

The difficult subject of relative stellar temperatures is treated with welcome originality in the work before us. Photographic evidence is set forth in it of the surprising wealth of solar stars in emissions of the shortest wave-lengths, relying upon which, the authors assign to those bodies the highest place on the cosmic heat-curve. They hold that the modifying effects upon spectra of temperature and density, being subtly intermixed, have been generally confounded, and that the prominence of spark-lines in white stars should be interpreted as significant of their tenuity, rather than of their enormous heat. It is certain, at any rate, as Lane of Washington demonstrated thirty years ago, that contraction is accompanied, up to a certain point, by a rise in temperature. Hence the "youngest" stars cannot be regarded as the hottest, since, although their store of energy is at a maximum, a large part of it is energy of position—thermal property, as it were, awaiting realization.

Changes in temperature and density are not the only factors in sidereal evolution. The progressive strengthening of surface-gravity in masses that pay by shrinkage for their expenditure of heat, has also to be considered. Great stress is laid by Sir William and Lady Huggins upon the results that must thence ensue in quickening atmospheric circulation, accentuating heat-gradients, and replacing, consequently, old by new orders of spectral phenomena. That they have hitherto received less than their due share of attention is scarcely surprising, since the conditions involved are highly complex and somewhat problematical. But that the cause is active and strongly influential, there can be no doubt whetever; so that theories ignoring it meet very imperfectly the exigencies of the situation. Already it seems as if the steady growth with age of gravitational efficiency might afford the solution to the enigma presented by the spectra of unequal double stars. But this is a topic not to be disposed of in a paragraph. The whole stellar scheme may be said to be implicated in it. Here we can only remark that the advances made at the Tulse Hill Observatory in the spectrography of coupled stars are beautifully exemplified in the 'Atlas' under notice. The contrasted spectra of the components of γ Andromedæ and β Cygni are especially remarkable, and we may hope that the disclosures they contain will soon be completed by visual observations in the lower spectral ranges, or by their delineation upon orthochromatic plates. This is the more desirable that the azure tint of certain star-satellites receives no explanation from the analysis of their blue light. We should then expect to find unusual absorption in the red or yellow; and bands answering to the colour-requirements of the object were actually seen in the smaller component of β Cygni by Sir William Huggins in 1864, and by Dr. Vogel in 1872.

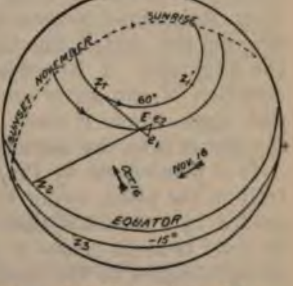
A vast amount of labour has been bestowed by our authors upon observations, both spectrographic and spectroscopic, of the great nebula in Andromeda. They are of a difficulty scarcely to be conveyed in words; only on the rarest occasions can anything quite definite be got out of them; still a decade of persistent toil has not been fruitless. The important conclusion has been reached that "the continuous spectrum of the Nebula, that is, of its *brightest* *central part*, contains dark lines of absorption, and also bright lines." The latter were, on November 15, 1897, seen clearly enough for the determination of the following wave-lengths: "580, 543, 528, 511 (a group of four or five lines), 495 (a group), 476, 455 (suspected)."

Now, all these "bright places" fall close to Wolf-Rayet lines. The coincidences are obvious, and have not, we believe, escaped the notice of Sir William and Lady Huggins, although, in the present unsettled state of the subject, they abstain from expressly adverting to them. The revelation is most curious, and suggests affinities startling to received ideas of sidereal relationships. But discussion of them would be premature until verification is at hand. And in our vaporous climate it may be long delayed. We can only rejoice that such advantages have not impaired the merits of the superb record issued from an Observatory half shadowed by London smoke-fogs. A. M. CLERKE.

The Opposition of Eros.

THE astronomical event of importance next to happen is the Opposition of Eros, when it is hoped that its parallax may be determined and the Solar parallax hence deduced with an accuracy of at least 0''·02. Opposition in long. will happen on November 12, when the distance of the planet from the Earth will be about 0·42 of the distance of the Earth from the Sun, but the bodies continue to approach one another until about 1901 January, when they will be separated by about 0·315 of the Sun's distance*.

Several papers have already appeared relating to observations of the planet, and, as will be seen from our report of the Astrographic Conference, a committee has been appointed to consider the best method of procedure. It may be expedient to set down here some points in the geometry of the subject, and in doing so we make due acknowledgments to a paper by Prof. Newcomb in Ast. Journal, No. 480, from which the following diagram with some additions is copied.



This diagram represents the celestial sphere projected on a plane perpendicular to the line from us to the planet, so that Eros (the geocentric position unaffected by parallax) is at E, the centre of the circle. If Z_1 be the point where the zenith of any place meets the sphere, and Z_1E be joined by a great circle, the planet will be seen from this place at e_1 on the prolongation of Z_1E, such that

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* See Observatory, vol. xxii. p. 370.

$$Ee_1 = \pi \sin Z_1E,$$

where π is the horizontal parallax of Eros. Similarly from another place whose zenith is at Z_2 the planet would be seen at e_2 where

$$Ee_2 = \pi \sin Z_2E\ ;$$

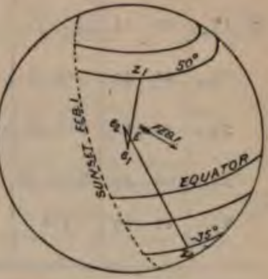
and supposing that photographs were taken simultaneously at Z_1 and Z_2, and the two plates superposed in such a way that the images of the stars were made to coincide, the two images of Eros would be separated by the quantity e_1e_2, which is the quantity, called the displacement, to be measured to find the parallax. The main question to be settled on arranging the programme of work will probably be the choice of the places Z_1 and Z_2, and the times of observation to obtain a displacement most suitable for measurement. As to an estimate of its amount, it can be readily seen that this depends on the distance between Z_1 and Z_2, the parallax π, and the zenith-distances Z_1E, Z_2E at observation. π at the beginning of October is about 15″, and the maximum on December 26 is about 28″; the zenith-distance naturally might be as much as 90°, so that displacements of 30″ or 40″ will often result from combination of observations.

It will be easily understood that if the diagram be taken to represent the celestial sphere as fixed, Z_1 (the zenith of a place about latitude 60°) describes a small circle, and instead of choosing a second station Z_2 from which to make observations, it would be equally possible to wait until Z_1 is at Z_1^1 say, and then make observation from the same place; or, again, the observations from different stations need not necessarily be made simultaneously, but it is better that they should be so, for if an interval of time elapses the reduction will include an allowance for the motion of the planet in the interval which it would be well to avoid. As the observations must necessarily be made when the sky is dark, in the diagram the dotted line shows the time of sunset and sunrise at the beginning of November—that is to say, Z_1 moving in the direction of the arrow crosses this line when the Sun is setting, and again when it rises. It will be noticed that at this time the planet is on the meridian about midnight.

It is pointed out by Prof. Newcomb that the planet will leave a trail on the plate, owing to its proper motion, if the telescope is guided by a star, and that for this reason it will be advantageous, in order to avoid errors of measurement, to make the displacement at right angles to the trail. It may be better to use the planet itself as guiding star, so that the stars leave trails; but the point is the same in both cases. Arrows are drawn showing the direction of motion in the middle of October and the middle of November; and with all these things in mind it is possible to consider the most suitable places for observation about this time.

At the middle of October simultaneous observations at Helsingfors at 16ʰ local apparent time (then at $Z_1{}^1$) and Arequipa at about 11ʰ (then at Z_2) will give a favourable position of the displacement with a length of about 25″. Earlier in the month simultaneous observations at Jamaica and Madras, which are both in latitude about 15° N., at the first-named place in the evening (left of diagram) and at Madras in the morning (right of diagram), would give an excellent combination. A month later such observations would not be suitable, it would then be more expedient to make simultaneous observations from places in the same longitude, but separated in latitude.

It is easy to arrange combinations such as these, and diagrams can be constructed for other epochs. We take another from Prof. Newcomb's paper, for 1901 Feb. 1, when the planet is on the meridian soon after sunset.



It is evident that it is not possible to get a long distance between the points of observation Z_1 and Z_2, since the planet sets about 6 hours after sunset. Here the maximum displacement (nearly 40″) would be obtained by observing simultaneously from two observatories, such as Helsingfors and the Cape; but in this case the displacement would not be much inclined to the direction of trail.

Lastly, in addition to the factors in the case above mentioned, there will have to be taken into account the quality of the instruments which each observatory commands. Harvard or Arequipa, with Greenwich, Paris, or Potsdam, might make a combination which on this account might be more suitable than another more favourable by reason of position; but this is a point for the Committee to decide. H. P. H.

Doubly-observed Meteors and Fireballs.

During the present year some important duplicate observations of meteors have been made, and I give a selection of 11 of these, as they seem of specially interesting character.

Date.	G.M.T.		Mag.	Height at first.	Height at last.	Path.	Velocity.	Radiant.		Observers.
1900.	h	m		miles.	miles.	miles.	miles.	α. °	δ. °	
Jan. 2...	11	58½	>1	57	40	44	22	228	+53	A. S. H., W. E. B.
25...	11	10	3–1	65	44	26	30	45	+62	A. S. H., J. H. B.
27...	11	10	1½–1	57	45	28	14	98	–10	A. S. H., A. K.
Mar. 28...	10	45	2–1	56	40	16	16	179	+47	A. S. H., J. H. B., A. K.
28...	11	29	3–2	73	65	18	36	252	+22	A. S. H., J. H. B.
April 21...	10	32	3½–2	60	54	50	17	188	–32	A. S. H., J. H. B., A. K.
July 15...	10	13	2×♀	51	21	78	16	297	–11	H. J. T., C. L. Brook, W. F. D.
19...	11	49	>1–♃	81	54	43	35	17	+50	A. S. H., W. F. D.
23...	12	12	2–1–♀	84	55	44	59	24	+52	A. S. H., W. F. D., W. E. B.
24...	10	49	♃->♀	68	27	103	19	280	–15	W. F. D. & others.
25...	11	10	3–1	75	48	46	35	8	+39	A. S. H., W. F. D.

OBSERVERS.—A. S. H., Prof. A. S. Herschel, Slough; W. E. B., W. E. Besley, London; J. H. B., J. H. Bridger, Farnborough; A. K., A. King, Leicester; H. J. T., H. J. Townshend; W. F. D., W. F. Denning, Bristol.

The meteor of January 2 was a Quadrantid. On May 3 an Aquarid (the first, I believe, whose real path has been determined) was seen with the usual great length of course common to the meteors of that shower. On July 19 and 23 early Perseids were recorded, and the observations are valuable as proving the position of the radiant at a period about three weeks preceding the date of maximum display. From very careful observations of the visible path of the Perseid of July 19 made by Prof. Herschel at Slough and the writer at Bristol, the radiant was indicated at 17° +50°, and close to the point 19° +51°, where the Perseid centre was determined at Bristol on July 19, 1887. This is the earliest Perseid of which duplicate observations have ever been forthcoming, and its radiant virtually settles the question as to the approximate place of the centre of the shower on the date mentioned. It is to be hoped that some further early Perseids will be recorded this year in the absence of moonlight during the last ten nights of July. There is no reason why at a future period we should not have accumulated the real paths and radiants of Perseids for every night from the middle of July to the middle of August.

A number of brilliant fireballs have already been seen this year, but the observations, as a whole, are not very exact. The principal ones appeared on January 9, 2^h 55^m, March 28, 8^h 31^m, May 5, 8^h 20^m, June 10, 9^h 10^m, July 15, 10^h 13^m, July 17, 8^h 47^m, July 24, 10^h 49^m, but large meteors were more or less imperfectly seen on many other dates.

Bishopston, Bristol, 1900, July 28. W. F. DENNING.

The Absolute Number of the Christian Era in the Julian Period.

A GREAT chronologist, Joseph Justus Scaliger, who was born at Agen (France) in 1540 and who died as Professor in the University of Leyden in 1609, founded with his work "De Emendatione Temporum Lutetiæ, 1583," a complete and regular chronological system.

In order to facilitate the comparison of dates, he introduced a period of 7980 years, beginning at a distant epoch from all historical events, and termed it the Julian Period, either on account of its being composed of Julian years, or in honour of his father Julius Cæsar of Riva, on the lake of Garde, who was physician at Venice, Padua, and Agen. By means of this period it is possible to compare dates which refer to different eras, so that it is only necessary to know the absolute number of each era—that is, the number of days comprised between the beginning of the J.P. and the beginning of the era in question. If we accept the computations of the early chronologists and church-writers of the 6th century for the supposed date of the Nativity of Christ, we arrive at the date Dec. 25, 4713 of the J.P. The year immediately following, 4714 of the J.P., was then called the year One of Christ, or 1 A.D., or also +1, while the entire year 4713 J.P. was called the year 1 B.C., or −1. Thus the old chronologists fixed upon the 25th Dec. 4713 J.P., as the commencement of the Christian Era—that is, as the zero of an enumeration extending in two opposite directions, −1 and +1, for the calculation of the years before and after Christ. But astronomers, for greater convenience and simplicity of calculation, put back this commencement by about a year to 0, Jan. 4713 J.P. This date is none other than the 31st Dec. 4712 J.P., and as the J.P. years are Julian ones with an intercalation of one day every four years, the 4712, when all completed, is comprised of 1178 quadrennial periods of 1461 days each $(4 \times 365 + 1)$; that is, taking the whole from the beginning of the J.P. to 4712, we count 1721058 days. This number, which determines the astronomical commencement of the era in the J.P., is called the Absolute Number of the Christian Era. It corresponds to the civil date 0.1.0, namely to 0 Jan. of the year zero. The supposed date of the Nativity of Christ, or the beginning of our civil era, occurred on the 359th day of this zero year; therefore it is evident that between the two commencements (viz., the civil and the astronomical) there is a difference of one year less the few days from the 25th to the 31st Dec., which, for the present, we may ignore, referring to them again later on. This difference, however, need cause no confusion in the designation of the years A.D., because astronomers call zero the year −1, so that +1 in the same for both calculations, and both proceed with the same denomination for the years after Christ. According to the civil calculation, therefore, the expression: "One A.D." implies the year that was next begun and completed after the birth of Christ, so that the words "Nineteen Hundred" signify that 1900 years or 19 centuries have been completed since the birth of Christ; thus the 31st Dec. 1900 will be the end of the xixth century A.D., and the commencement of the xxth. If astronomers, starting from the absolute number, were to compute time by centuries, they would necessarily say that 19 centuries had elapsed on 31st Dec. 1899, or that on this day, which corresponds to 0 Jan. 1900, the xxth century has begun. Thus every beginning of a hundred years expressed in Arabic numerals would be the beginning of a century, and precisely of that century which takes its name from the Roman numeral greater by a unit than the Arabic. But in astronomy the unit "century" does not exist, the greater intervals of time being computed in days. For this reason there is in the Nautical Almanac a table of the Julian Period, which gives the number of days of the J.P. for whatever date, beginning from 0 Jan. of the year zero, which, as we have seen, corresponds to −1 of the old chronologists or to 4713 of the J.P. In front of the zero is the Absolute Number, then under the first head come the successive quadrennial periods, and under the second numbers which gradually exceed 1461 days, excepting the quadrennial period 1580–84, where the difference is only 1451 days, owing to the Gregorian Reform of the Julian Calendar, and excepting the quadrennial periods following the centurial, not the bissextile, years, where the difference is 1460 owing to the same Reform. Zero, the sign or Arabic cipher, which stands for "naught," or the beginning of something, is also a sign denoting position or place, that is, the empty place indicated by ciphers. In the case of the civil date 0.1.0 (equivalent to 0 Jan. 4713 J.P.), zero, standing in the place of the days, indicates the beginning of the year, as well as the missing or still uncompleted days, and, standing in the place of the year, indicates the missing or still uncompleted years. Now, writing this date in decimal fractions of the year, up to the fourth figure, we have $0^{y}{\cdot}0000$, and writing the other date, 25th Dec. 4713 J.P., the day of Christ's Nativity, we have $0^{y}{\cdot}9847$. The first fraction indicates the astronomical commencement of the Common Era, while the second one indicates the civil commencement, counted from the first, namely from the beginning of the year zero. It is easy to see that the second fraction also indicates the exact difference between the two commencements, a difference but little short of a whole ($0^{\mathrm{y}}{\cdot}0153=6$ days), namely, of the year which separates the two commencements.

A. Abetti.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

August Meteoric Showers.

Gentlemen,—

As the Moon will be full on the evening of August 10 this year, the maximum of the Perseids is not likely to be a brilliant one from an observational point of view. It will be interesting, however, to watch for large meteors and to record their apparent paths in the hope of securing duplicate observations of the same objects observed at other stations.

For many years the θ Cygnids have formed a very strong annual shower contemporary with the August Perseids, but moonlight will probably efface this radiant in 1900, though it continues in play until August 21, and a few of its later meteors may be observed, for the effect of the Moon's presence will greatly diminish during the third week of the month. I have observed these Cygnids as follows :—

Date.	Radiant.		Meteors.	
	α.	δ.		
1886 July 27–Aug. 11..	291°	+51°	14	D. 314
1893 Aug. 4–16	292	+53	28	
1885 Aug. 4–17	292	+52	4	D. 407
1877 Aug. 10–16......	292	+48	7	D. 404
1893 Aug. 14	294	+51[*]	..	
1887 Aug. 14–21......	297	+55	6	D. 415

Heis appears to have been the first to ascertain this radiant, for his catalogue contains a position at 295° +54° for August 3–21 from 156 meteors (1847–74).

In 1879 August 21–25 I recorded 56 slow, bright-trained meteors from a radiant near *o* Draconis at 291° +60°. This rich shower of Draconids has not exhibited any conspicuous activity since the date named, but it should be carefully looked for when the epoch comes round every year, as it is likely to be a periodical display which will reappear with considerable strength at a future time.

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[*] Radiant of a doubly-observed shooting-star recorded at Bridgwater and Bristol.

Yours sincerely,

Bishopston, Bristol, 1900, July 16. W. F. Denning.

A previous Total Eclipse at Navalmoral.

Gentlemen,—

The question as to what was the previous occasion on which the shadow passed over Navalmoral may occur to some of us who witnessed the late eclipse there. As that of 1706 seems to have gone northward of this point, we shall probably have to go back exactly 300 years for the desired conditions, to July 10, 1600. Under what different circumstances would this be witnessed by the simple peasantry from that of 1900, when it is reported that 20,000 railway tickets were sold in Madrid for eclipse-excursionists!

The eclipse of 1600 was observed by Kepler as an obscuration of half the solar disk. Tycho ("in arce Benatek") also saw it a few miles east of Prague, and considered the magnitude 5 digits, middle $1^h\ 46\frac{1}{2}^m$. It occurred the year before his death.

The eclipse of 1842 must have been total over Madrid and possibly at Navalmoral; but as this was little more than 20 minutes after sunrise, it can hardly be reckoned among the list of total eclipses available for observation.

Faithfully yours,

Melplash Vicarage, Bridport,
1900, July 9. S. J. Johnson.

Edmund Weaver.

Gentlemen,—

Dr. Stukeley, in the *Philosophical Transactions* for 1754 (vol. xlviii. p. [221]) speaks of Edmund Weaver (whose acquaintance he had made in Lincolnshire) as "a very uncommon genius, who had made himself master in astronomy and was scarcely to be accounted the second in the kingdom," and adds that when his tables were shown in MS. to Halley, the latter suspected that he must have seen his own. Yet it does not appear that Weaver's were ever published, or anything of his except a short series of Almanacs (of which I have been able to find only that for 1745) under the title "The British Telescope." Is anything more known of this "uncommon genius"? Stukeley constructed a map, from the calculations of Weaver, of the shadow-path of the eclipse of May 28, B.C. 603, and contends strongly for its being the eclipse (commonly called that of Thales) which, according to Herodotus, put a stop to a battle impending between the Medes and Lydians. I need not again go over the subsequent investigations which are decisive for the eclipse in question being that of May 28, B.C. 585. But it may be worth while to call attention to the positive way in which Stukeley accuses Pliny of error in regard to the date of this eclipse, though that assigned by the latter seems to be perfectly correct when the right eclipse is taken. For Pliny (Hist. Nat. ii. 12) says that the year when the eclipse put a stop to the battle was the fourth year of the forty-eighth Olympiad; and this is interesting as a confirmation of the view that the first year of the first Olympiad corresponded to B.C. 776, as Clinton places it. For if B.C. 585 were the last year of the 48th Olympiad, and B.C. 584 the first year of the 49th, 48 complete Olympiads would have elapsed since that which is reckoned as the first, and $584+4\times48=776$. The eclipse furnishes therefore an important confirmation of the accuracy of the received system of ancient chronological dates. Weaver, however, seems to deserve great credit for his tables if really original; for the path of the eclipse of B.C. 603 mapped from them by Stukeley does not differ much (though somewhat more to the west, crossing the eastern Mediterranean instead of the northern Red Sea) from that derived from modern tables by Oppolzer.

Yours faithfully,
W. T. LYNN.

Blackheath, 1900, July 6.

Occultations of δ Scorpii.

GENTLEMEN,—

The occultation of the above-named star last night was little affected by parallax here as compared with Greenwich, though the duration was short. The disappearance must have taken place between $11^h\ 21^m\ 50^s$ and $11^h\ 27^m$, when a cloud covered the Moon. The emersion was well observed, exactly at the lowest point of the Moon's disk, at $11^h\ 53^m\ 5^s$. Power used 50 on 3⅜ inches.

A slight interest is attached to occultations of this star from the fact that it is the first one of which an occultation is recorded in the years Anno Domini among the Chinese observations found by Gaubil, and published in the 'Connaissance des Tems' for 1810. The mode in which the Chinese have recorded this bears an air of truthfulness:—

"An. 32 = 8^e^ anneé Kien-von, 4^e^ lune, jour sin-oney (12 mai) la lune pres de l'etoile δ du Scorpion. La lumière de la lune cachait la vue de l'etoile."

The Moon at the time was nearly full.

The Moon is also mentioned, in the same records, to have occulted this star on August 9, 821, and on July 6, 1001.

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage, Bridport.
1900, July 9.

OBSERVATORIES.

Cambridge University. — From the Report made to the Observatory Syndicate for the year ending 1900 May 25 we learn that 2565 observations of both Right Ascension and Declination were made on 105 nights with the meridian circle, 52 observations of nadir-point, 29 of collimation, and 28 of level. The stars observed were those of the A.G.C. Catalogue (Cambridge zone) which required re-observing, heliometer comparison stars observed at the request of Dr. Gill, and occultation stars observed for Col. Tupman. Besides these, observation has been begun of a zone of stars five degrees in breadth round the ecliptic, the accurate places of which it is thought will be a useful contribution to astronomy. This work is under the charge of Mr. Graham, who has made several little time-saving improvements in the method of reduction, and by their use two computers have been able to keep the reduction fairly abreast with the observing. The work with the Sheepshanks photographic equatorial now in course of construction, which is in charge of Mr. Hinks, has consisted mainly of experiment and alteration. Among other things the mirror has been resilvered, and Dr. Common has kindly undertaken to refigure this.

The Newall Telescope has been used for observation on 109 nights in the course of the year. The atmospheric conditions have been, for spectrographic purposes, much better than in the three preceding years. During the first two months of the period to which the Report refers the Bruce spectroscope was used as before in taking photographs of stellar spectra for the determination of velocity in the line of sight. This series was then considered completed, and the spectroscope, which hitherto had a single prism, was converted into a four-prism instrument, and with this, during the last nine months, 102 photographs of the spectra of bright stars have been taken. "The scale of the spectra is approximately 1·5 tenth-metre per revolution of the micrometer, or about 6 tenth-metres per millimetre. These are obtained with a camera whose effective focal length is about 1000 mm., and whose aperture is *f*/20, and in spite of the fourteen inches of glass in the total base of the prisms, sufficient light gets through the prisms in about an hour's exposure to give spectra of the brightest stars of density enough for accurate measurement."

Spectra of fifteen stars, which is the complete number chosen for the special investigation in view, have been photographed. The first interesting result derived from these, the binary character of Capella, has already been announced. When the spectroscope was dismounted for use in Algiers during the eclipse, a camera was mounted on the equatorial in its place in order that certain double stars, and in particular Castor, might be photographed with enlarged images, as spectroscopic observations at Cambridge and

Poulkova show that periodic fluctuations in the separation of the components of Castor with a short period of three days may be expected. Up to the date of the Report, June 5, the weather had been unfavourable for observations of this kind.

ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—In his Report for the year 1898 Dr. Gill stated that the 24-inch object-glass of the McClean equatorial would have to be returned to Sir Howard Grubb for rectification of certain faults. In his Report for 1899 he states that it was sent to Dublin on 1899, Oct. 21. Since that date the 18-inch guiding telescope has been used for micrometer observations of double stars.

The new Transit-Circle Observatory has not yet been received, and Messrs. Troughton & Simms were asked not to forward the instrument until the building was shipped. The old transit-circle was used vigorously during the year, 9450 transits having been recorded and 8947 observations of zenith-distance. These complete the meridian observations of the Standard Stars required for the Cape Zone of the Astrographic Catalogue, Dec. −40° to −52°. With the heliometer many observations of position relative to comparison stars have been made of each of the major planets at opposition. The number of complete measures of Mars was 18, of Jupiter 40, of Saturn 10, of Uranus 49, and of Neptune 33. Sir David Gill says that a single set of such measures gives such accuracy that it is necessary to take special precautions to secure adequate accuracy in the positions of the comparison stars. To this end a heliometer triangulation of all the stars used will be made to supplement the meridian observations of them made at other observatories.

For the Astrographic Chart, 152 plates, every star being represented by three images, each of 30 minutes' exposure, have been taken; 184 revision-catalogue plates have also been taken. These revision plates are a second series to replace the first, which H.M. Astronomer thinks it expedient to make. One hundred and twenty-six plates have been measured.

The volumes published recently by the Observatory and the state of the geodetic work under Sir David's direction were detailed by him at the meeting of the Royal Astronomical Society reported in our June number. To this it may be added that a Survey of Cape Colony and British Kaffraria made by Capt. Bailey, R.E., in 1859–62, has been revised, and the new work is in the press.

Four members of the staff who are members of the Cape Volunteer Artillery have been called out for service in the war, and this has necessarily increased the duties of others. Sir David Gill expresses his appreciation of the efforts of his staff and specially mentions the voluntary help rendered by Mr. Innes in connection with the Cape Durchmusterung.

NOTES.

Comet Notes.—In a recently published volume (vol. xxxii. part 2) of the Harvard 'Annals' Prof. Pickering has an interesting discussion of the photographs obtained of Swift's Comet (1892 I.). This comet was one of the most brilliant of the last decade, and the photographs, of which nine are here reproduced, show much interesting detail. The head appears as a large oval, while the tail consists of numerous slender straight rays diverging like a fan.

The tail is stated to have consisted of two parts, the inner, springing from the nucleus, being over 20° in length, and consisting of two narrow, bright, absolutely straight rays.

The outer tail sprang from the outer envelope of the head, was fainter, shorter, and more divergent, and marked by deep and wide rifts between the rays. In fact the structure of the comet resembled on a smaller scale that of the six-tailed comet of 1744.

The photographs of the inner tail showed periodic changes, which Prof. Pickering explains by a rotation of the comet about an axis longitudinally through it. The indicated period of rotation was about four days, and this was confirmed by photographs taken by Dr. Wolf and Prof. Barnard. It is pointed out that Dunlop suggested a rotation of the comet of 1825 in 19½ hours, and Bessel a rotation of Halley's Comet in 110 hours.

The rotation is explained by supposing the jets of the tail to be electrically repelled from the Sun, in which case their mutual repulsion would set up rotation of the kind described. The rotation would be in one direction to the north of the Sun's magnetic equator, and in the other to the south of it.

A bright region in the tail shown on some of the April photographs was carefully measured, and its distance from the nucleus found to be :—

April 5	....	570,000 miles.
,, 6	....	1,560,000 ,,
,, 7	....	3,680,000 ,,
,, 8	....	6,460,000 ,,
,, 10	....	16,000,000 ,,

Assuming this recession to be due to solar repulsion varying as the inverse square, the repulsive force is computed to be 39 times the gravitational force. This bright region showed lateral motion agreeing with the rotation in about 4 days already referred to.

Referring to Prof. Bredichin's theory of comets' tails, it is deduced that the tails of Swift's and Rordame-Quenisset's Comets were composed of a substance whose density was less than half that of hydrogen.

Then follow some remarks on the aurora, which is considered as a tail to the Earth, produced in the same manner as those of comets. From the auroral analogy Prof. Pickering thinks it probable that comets which pass perihelion at sun-spot maximum have more chance of developing a fine tail. It will be remembered that Mr. David Packer some years ago wrote a paper in which he stated that bright comets were more frequent at sun-spot maximum, and this, if it be a fact, would be explained by Prof. Pickering's theory.

A bright comet with a tail (*b* 1900) was discovered by Brooks at Geneva (U.S.A.) on July 23, its place on July 23^d 18^h G.M.T. being R.A. 2^h 43^m 40^s, N. Dec. 12° 30′. It was moving northward.

It was independently discovered by Borrelly.

Elements and ephemeris for Berlin midnight, by J. Möller.

T ……	1900 Aug. 3·30 Berlin M.T.
ω ……	12° 30′
☊ ……	328 2
i ……	62 36
log q ……	0·0064

	R.A.			N. Dec.	
	h	m	s	°	′
Aug. 1 ……	2	53	52	38	31
3 ……	2	57	12	44	37
5 ……	3	1	8	50	29
Aug. 7 ……	3	5	55	56	2
9 ……	3	11	45	61	12
11 ……	3	18	50	65	57

A. C. D. C.

Minor Planet Notes.—Signor Millosevich has computed (*Ast. Nach.* 3650) new elements of (433) Eros with the aid of the recent observations. Applying perturbations he deduces the following values for the oppositions of 1898 and 1900:—

Epoch and osculation…	1898 Aug. 2·5	1900 Oct. 31·5 Berlin M.T.
M ……	205° 21′ 14″	304° 25′ 3″
π ……	121 11 3	121 9 59
ω ……	177 39 19	177 39 21
☊ ……	303 31 44	303 30 38
i ……	10 49 29	10 49 33
ϕ ……	12 52 6	12 52 32
μ ……	2015″·330	2015″·295

The following outline ephemeris is for Berlin midnight:—

	R.A.			N. Dec.			R.A.			N. Dec.	
1900.	h	m	s	°	′	1900.	h	m	s	°	′
July 21....	1	20	41	19	17	Oct. 9....	2	43	12	47	52
Aug. 10....	1	51	55	25	53	19....	2	35	48	51	5
30....	2	19	25	32	56	29....	2	21	4	53	25
Sept. 19....	2	39	12	40	24	Nov. 8....	2	1	21	54	22

The planet is nearest to the Earth on Dec. 26, when its distance is 0·3148, and parallax 27″·96.

Ast. Journ. 483 contains a paper by Mr. H. N. Russell on the great inequality of Eros and the Earth. He finds for the perturbation in mean longitude of Eros

$$+747'' \sin(7g-4g')+100'' \sin(14g-8g'),$$

where g, g' are the mean anomalies of Eros and the Earth.

The period of the first term is 41·24 years, and the whole range of its effect on the planet's place as seen from the Earth is nearly 3°. We may thus hope to eventually deduce the Sun's parallax by this method with greater accuracy than by any other. The results given above are only approximate, as some abbreviations were introduced into the calculation.

Planet (444) has been named Gyptis. A. C. D. C.

A METEORIC THEORY OF THE GEGENSCHEIN.—Of the many theories that have been put forward to account for the phenomenon of the Gegenschien, several ascribe the light to the existence of meteors or asteroids. The latest research of this kind is by F. R. Moulton in the *Astronomical Journal* for 1900 June 14 (No. 483), who attacks the subject mathematically. The mathematics cannot be well abbreviated here, but the scope of the paper can be judged from the following extract:—

> It is proposed to show in the following paragraphs that it is possible for meteors to move for long periods of time in the vicinity of a point in opposition to the Sun, and about 900,000 miles distant from the Earth. The suggestion will then be made, as being a reasonable hypothesis, that the meteors revolving around this point are sufficiently numerous to cause the faint glow of the Gegenschein by reflecting the light of the Sun.
>
> The space around the Sun occupied by the planets seems to be teeming with meteors, at least as far out as the Earth. They are indeed so numerous that the late Professor H. A. Newton calculated that ten millions strike into the Earth's atmosphere daily. They seem to be the cause of the zodiacal light, and they doubtless crumpled and destroyed the tail of Brooks's comet in the remarkable manner shown in Barnard's excellent photographs of this object. From observations they have been found to move with widely different velocities, and in every direction, so that considering their countless numbers it does not strain our imagination seriously to suppose that a great multitude would get in the vicinity of the opposition point, and be near enough some of the triple infinity of possible initial conditions so that they would remain there for a long time.

This hypothesis evidently explains qualitatively the established phenomena of the Gegenschein. Quantitatively the question seems to be beyond the power of mathematical treatment, and individual judgments regarding the efficiency of the theory will doubtless vary widely.

After this follows mathematical investigation, which shows that meteors are captured by the Earth and congregate in the zodiacal band in opposition to the Sun, that their orbits are unstable and hence a continual supply of material is required, that if these are the cause of the Gegenschein, it should have a monthly oscillation in longitude as observed by Douglass but less in extent, and that the oscillation in latitude would be monthly instead of yearly, as the observations seem to indicate.

There is another phenomenon of a striking character which has been observed by Barnard alone so far as I am aware. It is that the Gegenschein undergoes marked changes in form in a comparatively short time. He has observed many times that it is large and round in September and about the first of October, becoming slightly elongated by the 4th or 5th, and very much elongated by the 10th or 11th, and a mere swelling on the zodiacal band by the 18th. I do not pretend to assign a cause for this phenomenon. It does not, however, seem to be necessarily contradictory to the theory of this paper, since the shape of the Gegenschein would depend upon the thickness of the zodiacal disc, and if the opposition point should pass through a dense meteoric stream it might change its form very rapidly.

After having written this memoir and sent it to the Editor of the *A. J.*, Mr. Moulton had his attention called to the fact that M. Gyldén had, in Tome i. of the *Bulletin Astronomique*, when discussing the Problem of Three Bodies, made this very suggestion as to a possible cause of the Gegenschein. The coincidence of ideas of two eminent mathematicians gives weight to the suggestion. It is, perhaps, well that Mr. Moulton was ignorant of the existence of the previous memoir.

COLOUR PHOTOMETRY.—Colour photometry is a subject that Sir William Abney has made his own, and in his last communication to the Royal Society he describes a method of estimating the luminosity of coloured surfaces that is especially applicable when the source of light is a large surface, such as the sky. In 'Colour Photometry, part iii.,' it was shown that only one ray of the spectrum, a greenish-yellow, progressed in luminosity at the same rate as white light. If, for example, red, greenish-yellow, blue, and white lights are made of equal luminosity, and the illuminating beams are simultaneously and equally reduced in intensity, the luminosity of the red will diminish the most rapidly, that of the blue the least rapidly, the other two remaining equal. Moreover, the colour disappears more quickly than the luminosity (except in the case of pure red), tending towards greyness, so that colours of feeble luminosity are more easy to match than bright colours. The new method of colour photometry is based upon these facts. By means of concentric rotating discs, which are, when necessary, slit radially and interlaced, the proportion of black and white that matches first a green and then a yellow disc is determined. The comparisons are facilitated by observing the rotating discs through a "black transparent medium," such as an unstained developed photographic film, which may be so dense that the colour practically disappears, giving place to a dull grey. The value of a red disc is ascertained by interlacing it with the green and blue discs to produce a grey, which is then matched with the black and white. Thus, having three standard colours of known values, the luminosity of any other colour can be ascertained by substituting a disc of it for one of the standard colours to produce a grey, and matching the grey as before. The results given by this method agree closely with those obtained by the method previously described by the author. Sir William Abney has in this way determined the luminosities of various coloured surfaces and calculated the amount of black necessary for each, so that they shall be reduced to equal luminosity. He has then prepared a disc divided into several annuluses, each partly coloured and partly black, so that when rotated the whole appears of equal luminosity when illuminated by the light for which it is calculated. By the selection of suitable colours such a disc is a very convenient and effective test for any defect in either the colour sensitiveness of a photographic plate, or in the coloured screen used to compensate its inherent deficiencies in this matter. For the rotating disc, which is equally luminous throughout, will give, when the negative is developed, an image of equal density throughout, if the sensitive plate and colour screen are properly adjusted to each other. [From *Nature*, 1900, July 19.]

DR. BRASHEAR ON RULING GRATINGS.—The 'Transactions of the Astronomical and Physical Society of Toronto' always contains a great deal of interesting reading. In the volume for 1899, Mr. Thos. Lindsay continues his History of the *Nautical Almanac*, with a chapter on star-catalogues, giving a brief account of some of these (not such an exhaustive list as Mr. Knobel's classic paper on the subject in the *Memoirs R. A. S.*) from Hipparchus to Maskelyne. The Society must have had an interesting meeting on the occasion when Dr. Brashear was their guest and spoke to them about the method of ruling diffraction gratings.

The Doctor's remarks relating to the diamond point itself, which does the ruling, were particularly interesting. It was said that all parts of the dividing engine are a fair field for science to work upon, but there is no science about the diamond. For instance, a gem may be selected which, under the microscope, has just the point required; it is tried and found useless; again, the diamond may not, by such tests as can be applied, appear to have its plane of cleavage just right, and yet it may work beautifully. Dr. Brashear, without offering any theory at all, stated that it seemed to him that there is just one crystal, poised upon another, which does the marking; but this may drop off by wear, and although another crystal is there, the evenness of the spaces is destroyed in that moment for that plate. An interesting circumstance was related in connection with the work of the late Prof. Rogers, of Maine. It appears that Dr. Rogers had accustomed himself to the sound made by the diamond point, feeble as it was, when cutting the lines, and he was able to tell when the machine was working properly, and could detect the very moment when the unfortunate changing of a crystal might occur. The truth of Dr. Brashear's remarks were clearly perceived when he stated that the commercial value of a first-class diffraction grating is never romotely an equivalent of the time and labour and worry expended upon its construction. Small gratings, however, may be made, the commercial value of which does more nearly approach the real value as measured by the energy expended upon them. The Doctor's characteristic words were, "when a man has ruled a perfect grating, there is no more conceit left in him."

THE GREAT HEAT AND DROUGHT IN JULY.—From reports which have been published in the public press it would appear that a heat-wave of exceptional severity has been experienced during the past month in the principal continental cities, and, indeed, throughout the whole of Europe. Similar reports reach us with regard to the prevalence of exceptional heat also in America; and that London has not been exempt from this visitation the following facts derived from the records maintained at the Royal Observatory will serve to show.

The earlier part of the month in London held out no immediate prospect of great and exceptional warmth, being, indeed, very cool, whilst, up to the 6th July, rain which had fallen on three days had credited the month's rainfall with 0·56 inch. A prolonged drought then set in, which lasted without intermission for twenty days, and which was accompanied, after the 10th July, by very hot and brilliantly sunny weather, the drought being finally broken up on the afternoon of the 27th July by a succession of violent thunderstorms, which yielded a welcome rainfall of 0·84 inch. During the eighteen days included between July 10–27 the recorded sunshine amounted to 203·6 hours, which is no less than 71 per cent. of the possible duration. On the 10th the record amounted to 15·3 hours, which is equivalent to 94 per cent. of the possible amount; on the 11th to 15·1 hours, or 92½ per cent.; and on the 25th to 14·6 hours, or 93 per cent. The heat during this period was phenomenal, the maximum shade temperature exceeding 80° on fourteen days, and exceeding 90° on four of these days. The highest recorded shade-temperature at the Royal Observatory was 94°·0, a temperature higher than any recorded in July in the preceding eighteen years. In 1881 a maximum temperature was recorded on July 15 of 97°·1, the highest on record in the 60 years 1841–1900. In 1868, on July 22, there was a temperature of 96°·6, and on 1876 July 17 one of 94°·0, and these, with the temperature recorded this year, are the only instances in July of temperatures reaching to or exceeding 94°. The average number of days in July, from 50 years' observations, on which the temperature exceeds 80° is six, so that the number experienced this year (14) is remarkable, but has been greatly exceeded in 1859, in 1868, and in 1852, when there were respectively 21, 20, and 19 such days. In 1868 there were five instances of temperatures exceeding 90° in July, and in 1881 four instances, so that July 1900 takes rank with these years of exceptionally high records. The maintenance of high temperature during the nocturnal hours is another point of interest as marking the exceptional character of this period, and it may be noted that the night-temperature on the 23rd did not fall below 65°·3, which is an unusually high night-temperature, and has, indeed, been exceeded on *one* occasion only in the preceding 59 years, viz. in 1872, when the minimum temperature was 66°·1. Finally the mean temperature for the eighteen days July 10 to 27 was 70°·8, being 8°·1 above the average. A similar period in 1868, extending from July 11 to 28, gave a mean temperature of 70°·7, and a period in 1859, extending from July 5 to 22, a mean of 70°·6.

A VIOLENT PARADOXER.—Mention has been made on former occasions of a gentleman—a professor and a natural scientist he calls himself—who has queer ideas of things. When we last heard of him he was threatening to prosecute the School Board of Portsmouth because the children were taught that the Earth is not flat. He now seems to be worried about the solar eclipse, for he cannot understand how the Moon can hide the Sun, which is so many times larger. He therefore condemns all astronomers in no uncertain language, and, moreover, writes to the Astronomer Royal about it. As his previous communications have been met with a chilling reserve, he concludes his letter thus:—"None of your dogged silence. Write and tell me you will alter or resign, or I'll make an awful row about it." There!

IN the list of recent visitors to Greenwich, of whom there are generally a good number from abroad at this time of year, are the names of Mr. and Mrs. Swasey, Dr. Elkin, Prof. D. P. Todd, Prof. W. H. Pickering, Dr. Thome on his way to the Paris Conference and a run round the Observatories of the Continent, and Mr. Putnam of the United States Coast Survey, who swung half-second pendulums in the Record Room.

FROM AN OXFORD NOTE-BOOK.

PH-H-H-H! But what heat formidable! Think of a Conference in Paris when even Reaumur approaches 32°, reminding the poor foreigners who live by Fahrenheit tantalizingly of delicious ice and snow! A Conference on star photographs? It is more like actual experiments on the absolute solar temperature. Can all this heat indeed come from the Sun, or has something gone wrong down below and ——? But it is too hot to think of this: let us rather consider the date of the next chance of something with ice in it, and meanwhile select for observation some one who is obviously feeling the heat more than ourselves; for this is a malicious but genuine comfort, and there are those even from hot climates who are clearly suffering and ready to acknowledge that Paris has provided something special for them independently of the Exposition. Practice does not always help one to bear heat, at least this is my experience. I remember when we were on the way to India, and it began to get a little warm in the Red Sea, a gentleman on board who was returning to his native land astonished us by sitting in the smoking-room with his sun-helmet on, declaring that if he took it off the Sun would pour down on him through the roof (a mere 3-foot of wood), and give him sunstroke. Those who knew the facts better than I did were not surprised. They explained that at his own home he would have punkahs and tatties and things to help him bear the heat, and thus was quite unaccustomed to grapple with it neat. If he had had to attend a Conference in a black frock-coat, it would probably have killed him dead.

BUT the black coat has been laid aside even at the Conference. On the first morning the courteous President opened the proceedings by giving us all leave to come without it in this exceptional weather; and some of us who had come without it already would have blushed if we could have got any redder in the face than we already were. The President addressed a numerous gathering: there were 40 good men and true to hear his opening remarks, though we missed one or two well-known faces. One of the absentees (owing, we were sorry to learn, to ill-health) was M. Donner, of Helsingfors, who had been M. Trépied's co-secretary in 1896, and whose place must needs be filled. Remembering his virtues, M. Tacchini suggested that we should represent him as nearly as possible, especially in name, by substituting M. Dunér; and this genial proposition was at once adopted.

Geniality and harmony have been characteristic of the meeting. We have managed to exclude the heat at any rate from the discussions. It is true that one proposition gravely propounded was forcibly characterized by an opponent as "first dangerous, and secondly useless, and what can we do with a proposition like that?" But these apparently aggressive words were uttered in such an inimitable manner that the only explosion which followed was one of laughter, and the proposer amiably accepted the situation. With this tone of friendliness, and able officers to guide the discussion, a great deal has been done in a few days. There are not many definite resolutions to show for it; but the time has gone by for resolutions; we have now been at work some time and have found a better guide in actual experience than any resolutions can give.

But perhaps it should be stated which Conference this is, as some others are being held in Paris this year—actually 453 others, I believe. This one is a meeting of the Permanent Committee on the Astrographic Chart, and the first thing to do was to hear reports from the 18 observatories which have undertaken shares in the work. Fifteen of them supplied reports showing that their work was well advanced; in the remaining three it had hung fire altogether. Fortunately this deficiency is in a fair way to be made up, two new colleagues being ready to promise help, and a third being within the bounds of possibility. The well-known name of the Cordoba Observatory is a welcome addition to the list. Mr. Thome had come over in person to promise his assistance, backed by the liberality of the Argentine Government, and was most cordially received. M. Enrique Legrand, from Monte Video, gave us the welcome intelligence that he had induced his Government to found a new observatory to help; and there seems reason to hope that a request to the Perth Observatory (W. Australia) may be favourably received. If so the list will be very satisfactorily completed again.

Then we all compared notes as to our intentions with regard to publication of the Catalogue, and found that practical unanimity could be secured by considering *immediate* intentions only. We were all ready to publish rectangular coordinates of the stars as measured, with constants for orientation, scale-value, centering, &c., and the equatorial coordinates (R.A. and Decl. 1900) of the stars used to obtain these constants; and in order to reap the benefits of this agreement the further question of publishing R.A. and Decl. for *all* the stars, on which opinion was not so homogeneous, was deferred for the present. I was not able to wait for the discussion on the publication of the Chart, which is a more difficult question. The beautiful charts being published by the four French observatories make it rather complex. There is no doubt about their beauty and desirability, irrespective of expense; *but* (and it is a rather large "but") to follow suit would cost each observatory about £10,000! It almost looks as though it is too great a luxury. For my own part, while earnestly hoping that the French, who find they can get the funds, will complete their beautiful piece of work, I feel that we must not be disappointed if others cannot carry it on over the whole sky. We have always the star-charts of the whole sky already made, and being continually made, by Prof. Pickering; and when the value of these is more generally realized, it may be found that the reproduction of the Chart plates is unnecessary.

But our attention was not exclusively claimed by the Conference of course. There was the Exhibition to see, and especially the big telescope. The telescope is there all right, complete in all details; and a gigantic piece of work it looks, worthy of the skill of M. Gautier. Unfortunately I had to leave Paris before the date fixed for the members of the Conference to see it under special conditions, and thus of its performance I can say nothing. But the "Salle d'Optique" is reinforced by a variety of other attractions, such as views of microbes on a large scale, realistic representations of the Earth during the days of creation, dancing girls dressed in phosphorescent robes, and finally a statue of Miss Adams, of New York, in pure gold—"the connection of which with the plot one sees." With reference to the phrase "la lune à un mètre," which, as the guide-book remarks, "a fourni prétexte à tant de plaisanteries," I find in this work the following explanation :—"Le grossissement dépasse 8000 diamètres, alors que les plus forts grossissements obtenus jusqu'ici n'atteignent pas 3500. La lune n'est pas rapprochée à un mètre, mais à environ 70 kilomètres, ce qui est dejà fort joli."

To turn to more serious matters, there are some splendid collections of astronomical photographs at the Exhibition. There is the danger in saying anything of them that one may have missed the best of all, as the Exhibition is such a huge place and they are scattered about rather. I can only speak of what I saw, viz., the Paris-Meudon, Greenwich, and Harvard photographs, all beautiful collections. M. Loewy's big Moons and M. Janssen's Suns show really well on the walls. Most of the Greenwich photographs are more or less familiar to those who attend the Visitation Days, but they are well exhibited in their foreign home. Let me warn those who go to look at the Harvard photographs that only a fraction of them are immediately visible on the walls. The front pulls open and reveals stacks upon stacks behind; it is an ingenious arrangement for showing a large number in a small space, but I don't think I should have found out the trick without competent assistance, and I should have been very sorry to miss them. One of those that interested me most was that of Eros on February 5, 1894, where the planet has made a huge trail. Now, to the best of my recollection, when it became clear from the approximate orbit that the planet must have been near us in 1894, back photographs were looked up for it without success; it was only after the planet had been found on the 1896 plates, and the orbit revised, that it was found on the earlier plates; and yet here it is so very obvious! Was this trail considered perhaps a photographic fault (being almost vertical) and so neglected? If my facts are right, this photograph is a most interesting comment on what may be overlooked because it is too obvious. Would Prof. Pickering be able to afford us a few historical words about so unique a case? I think they would be received with very great interest. This plate seems (from his account in *Ast. Nach.* 3545) not to have been examined in the first instance, which is another possible explanation.

At the Paris Observatory we were generally so busy talking that we had not time to look at the novelties; but one of them was tolerably conspicuous—a horizontal tube as large as that of the big telescope in the Exhibition. M. Loewy has enclosed the space between the transit-circle and the meridian mark by this tube of thick felt, and finds an eminently successful result, viz., that the image of the mark is quite steady now instead of being always boiling or jumping. The hint is eminently worth taking at observatories where such a tube can be erected without opposition from an æsthetic public.

A letter just received from Prof. Barnard gives the welcome intelligence that all his eclipse photographs have come out well. He sends me a print of the part of the limb showing the three prominences in the S.W. quite wonderfully. And he says that "the eclipse plates also show *stars* between 6 and 7 magnitude—possibly fainter, I have not carefully examined yet." This is something new?

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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Total Solar Eclipse, 1900 *May* 28.

Note on the Observations made at Algiers *.

The Council of the Royal Astronomical Society having generously offered assistance for me to proceed to Algiers to observe the eclipse, Professor Turner wrote to M. Trépied, Director of the Algiers Observatory, asking if he could place an instrument at my disposal. M. Trépied cordially agreed, and in a very courteous manner placed the $0^{\mathrm{m}}\cdot318$ equatorial coudé at my disposition. Being naturally anxious to make the best possible use of the instrument so generously confided to me, I determined to examine in minute detail the lower regions of the corona.

As I had had occasion to study minutely the form and structure of the corona as photographed in the eclipses since 1870, it seemed desirable to compare the aspect of the corona as photographed and as observed visually. There was a probability that the eye would see details not shown in the photographs. My attention had been recently directed to the point in a letter from Prof. Langley on the eclipse of 1878. In his 'Report to the Superintendent of the United States Naval Observatory' he says:—

> Now what I saw in this brief view was a surprisingly definite filamentary structure somewhat coarser and decidedly more sharply defined than I have ever seen filaments in the photosphere, not disposed radially, or only so in the rudest sense, sharpest and much the brightest close to the disk, fading rapidly away into invisibility at a distance of five minutes of arc or more (possibly in some cases of ten).

Prof. Langley then remarks:—

> Interesting as may be the photographs of the interior structure of the corona which have been recently obtained, this structure has not yet been completely studied, even on the best photographs I have seen; the means at our disposal,

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* *Comptes Rendus,* 1900, July 23.

photographed corona, believing that there existed details visible to the eye which were not reproduced by the photographs. I do not think this was the case in the 1900 corona. I do not believe I saw more details than are shown in the photographs; and now maintain that good photographs, taken on a sufficiently large scale, are capable of showing all the details which can be distinguished by the eye at the telescope. I make this statement the more readily as it disagrees with my preconceived ideas. I had expected to see more or less detail of a complex nature, and my attention was especially directed to this point. On the other hand, the sky was absolutely transparent, and the instrument which I used was, without doubt, better than any hitherto employed for visual observation of an eclipse.

The opinion often expressed that the details visible to the eye are more numerous and complex than those shown on the photographs, is based, it seems to me, on an imperfect recognition of the details.

In 1872 I saw, for the first time, the fine photographs on collodion plates of the 1871 corona, obtained by Mr. Davis for Lord Lindsey. Mr. Davis told me then that the corona, seen at the telescope, showed infinitely more detail than was shown on these plates.

At the moment I had not studied these plates, which since have occupied me some months; I am quite certain that if Mr. Davis or myself had then properly counted the number of details on these very plates, we should have found hundreds more, distinct although faint.

Mr. Brothers, who confided to me his 1870 eclipse plates, had not noticed, as he afterwards assured me, the large number of details since found on these plates. This may also be the case with the American eclipse plates of 1878. The positives which have been sent to England show very little detail, and the drawing published from the photographs in the 'Washington Observations, 1876, Supplement III.,' evidently does not show all the original detail, as can be seen by comparison with one taken by Mr. Ranyard. This plate is not good, the development has been forced; it nevertheless shows numerous details not in the Washington drawing, and which I cannot distinguish on the positives.

It is desirable that the plates obtained by the various American missions in 1878 be submitted to a critical examination. Of course the original plates are absolutely indispensable; no reproduced photograph would show the details, the most delicate structures being invariably lost.

The best results, so far as details of the lower region of the corona, were obtained on the large-scale photographs of 1893.

For the study of limited areas, always little illuminated, the best photographs are taken with small instruments and rapid plates. It is almost unnecessary to say here that a series of plates obtained with varying exposures is required, because a single plate never gives all that is visible.

The study of the detail in the lower portion of the corona will enable us to decide whether the corona is formed of an interior and exterior corona, distinct or not. If the structure of the lower region is only the base of the long rays analogous to those of the outer regions of the corona, but foreshortened by reason that they are seen at different angles, it is evident that the corona is really *one*, and that the distinction between the interior corona and the exterior corona (a distinction on which some insist) is purely arbitrary and ought to be abandoned—a conclusion which, so far as concerns the 1900 corona, appears to me highly probable.

W. H. WESLEY.

A Geodetical Enterprise.

THERE is some uncertainty as to the exact date when mankind generally began to realize that the surface of our Earth is not a flat plane. Thales (B.C. 600 circa) and Pythagoras (B.C. 550 circa) are both said to have taught that the planet is a sphere, but Eratosthenes (B.C. 276) is supposed to have been the first geodesist, and to have reduced this principle to practice, for he noted that on the day of summer solstice at noon objects cast no shadow, whereas at noon of the same day at Alexandria the Sun was in such position that shadows were seen on the ground, and from the measured dimensions of the shadows and the distance between Syene and Alexandria, the length of the Earth's circumference was evaluated. Passing by the dawn of geodesy marked by the knowledge of this elementary fact of the sphericity of our planet, we come to the next fact in the science, namely, that the Earth is not an exact sphere, but is flattened at the poles like an orange, as the primers say.

Here we are on more certain ground as to date, for it was Newton who first suggested, purely from theoretical knowledge of the results which follow from rotation of a comparatively plastic body, that the form of the Earth is an oblate spheroid, its equatorial diameter being longer than the polar. He was able to have soon a proof of the truth or error of this theory from direct measurement, for a geodetical campaign was then in progress at the hands of Domenico and James Cassini, father and son, with Lahire and Maraldi. Picard, who was the first to put spider-lines in the focus of a telescope, and so to use it as an accurate instrument, having measured a base-line had deduced the length of an arc of about a degree and a third between Amiens and Paris, from which the dimensions of the Earth were computed,—it may be noted in passing that this result of Picard's, which has since been found to be very nearly the truth, as judged by more modern work, happened to be the very information necessary for Newton to prove his hypothesis of universal gravitation—but it was felt that to deduce the size of the whole Earth from such a comparatively small arc was not a satisfactory method, and therefore the persons above named were extending Picard's arc northwards to Dunkirk and south to Perpignan to cover about 8½°.

In view of Newton's statement that the terrestrial spheroid is oblate, this survey was of much interest, for if this be the form of the Earth, the measurement of a northern arc should give a greater linear value of a degree than a southern, since the circle of curvature of an ellipse is greatest at the end of the minor axis and least at the ends of the major, so that when Cassini, in 1718, published as the result that the northern part of the arc of 2° 12′ amplitude gave the length of a degree as 56,960 toises, while the southern part of 6° 19′ amplitude gave 57,097 toises as the length of a degree, hence implying that the Earth is a prolate spheroid, some excitement was caused in scientific circles, a certain international feeling being introduced into the discussion, since it was a question of French practice against English theory. The French, nevertheless, did not put a bigoted confidence in their measures, but resolved to put the question to a crucial test, and the Academy of Sciences organized expeditions to measure the length of a degree at the Equator and another at the Polar Circle. The first part of the project was carried out by MM. Godin, Bouguer, and De la Condamine, who, during several years beginning in 1735, measured an arc of 3° 7′, which intersected the Equator near Quito, in Peru. A second party, led by Maupertius, went to Lapland in the year 1736, and in sixteen months measured an arc of the meridian about 57′ long. The measurement of the arc at Quito gave 56,749 toises as the length of one degree, whereas Maupertius found 57,437 toises to be the equivalent of the same arc, which negatived the assumption that the polar diameter is longer than the equatorial. It may be added that Cassini de Thury, son of the James Cassini above mentioned, with Lacaille, revised his father's work and announced in 1744 that it agreed in showing the spheroid to be oblate.

This is, in brief, the early history of our knowledge of the figure of the Earth, omitting everything connected with pendulum experiments. It was part of Newton's theory that as the polar diameter is less than the equatorial, a pendulum would lose time when taken to the Equator, which was verified accidentally by Richer in 1672, and afterwards by others, who took clocks to southern stations to make such investigations. But this short account is written here introductory to the statement that there is now on hand a project—the work, indeed, is actually begun—to make re-measurements of the arcs measured by Maupertius and by La Condamine in the first half of the eighteenth century.

It will have been rightly judged that earth-measurement was at first almost exclusively a French science. In the beginning of the nineteenth century it was taken up with vigour by Germany under

Gauss and Bessel, by Russia, and by England at the hands of the officers who had charge of the Surveys of Britain and India. Later the United States Coast Survey was established, so that there are available at the present time as data for accurate determination of the figure of the Earth the measurement of the arcs in the following list, which occurs in a recent report on the above project to the Paris Academy of Sciences by M. H. Poincaré * :—

Arcs of Meridian.

(1) The Anglo-French arc from Laghouat (lat. 32° N.) to Shetland (60° N.).
(2) The Indian arc between the latitudes 8° and 32° N.
(3) The American Atlantic arc between latitudes 32° and 45° N.
(4) The American Pacific arc between latitudes 30° and 40° N.

Longitude Arcs.

(5) The European arc from Valentia to Omsk, about 52° N, latitude.
(6) The arc across the American Continent in latitude about 38° N.
(7) The arc across India in latitude about 24°.

No doubt Dr. Gill will be able to make a valuable addition to this list in a few years; but M. Poincaré points out that these are all in the northern hemisphere and in mean latitudes: in equatoreal and polar latitudes nothing has been added to the work of last century. He goes on to say, "This is much to be regretted, for these ancient determinations, remarkable as they were for their epoch, evidently cannot be compared with recent work."

This has been the feeling of geodesists for a long time, and the matter has been broached at various meetings of the International Geodetic Association. At the meeting in 1889 the delegate from the United States brought forward the question, but, though part of the operations lies more nearly at home to them geographically, the American geodesists evidently feel that France has a prescriptive right to the honour and glory of this work.

France has not shrunk from the task, but political conditions in South America made it for some years impossible to proceed with a scientific operation which required the goodwill of those in authority. In 1898 these conditions were altered, General Alfaro, whose help could be reckoned on, was elected President of Peru. The French Minister of Instruction provided a sum of 20,000 francs for the purpose, and two officers of the Service Géographique de l'Armée, MM. Maurain and Lacombe, spent some months of 1899 in Peru making preliminary arrangements, selecting new stations and territories for bases, for the old arc is to be extended about one degree to the north and two degrees to the south, so the re-measurement of this may be considered actually begun. The work is to be done, as it was in 1735, under the auspices of the French Academy, but actually it will be under the direction of General Bassot, of the Service Géographique, and will be done by officers of that organization.

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* 'Comptes Rendus de l'Académie des Sciences,' 1900, July 23.

The other part of the investigation, the re-determination of the arc measured by Maupertius, has been taken in hand by Russian geodesists. Some months ago a Russo-Swedish expedition started for Spitzbergen, and it is believed that the measurement is in progress. H. P. H.

Doubly-observed Meteors and Fireballs.

As the Aquarid of May 3 was omitted from the table in the *Observatory*, p. 314, I have included it in the following additional list of some of the many interesting meteors observed at more than one station this year:—

Date.	G.M.T.	Mag.	Height at first.	Height at end.	Path.	Velocity.	Radiant.	Observers.
							α. δ.	
1900.	h m		miles.	miles.	miles.	miles.	° °	
April 20...	11 44	1	70	63	53	35	210 −31	A. S. H., J. H. B., & others.
May 3...	13 58	5–2	54	49	155	28	337 ± 0	A. S. H., J. H. B.
July 17...	8 47	Fireball	58	15	175	slow.	249 ±20	17 obs.
18...	11 33½	♃	52	50	45	11	214 −10	A. S. H., H. S. Campbell.
23...	11 13	2–1	83	59	49	30	30 +47	A. K., W. F. D.
23...	13 1	2–1	67	66	51	swift.	54 + 9	W. E. B., W. F. D.
24...	12 17	4–1	78	71	39	49	14 − 6	W. E. B., W. F. D.
28...	11 46	3–1	65	44	52	29	337 −10	A. S. H., W. F. D.
30...	11 45	3–2	56	40	52	35	338 −10	A. S. H., W. F. D.

The fireball of July 17 was a splendid object, but it appeared in strong twilight, before the stars were visible, so that its path could not be very accurately ascertained. The bright meteor of July 18 was remarkable for its sinuous or wobbling movement, and it appears to have had a very slow rate of speed. The real path here given is widely different to that mentioned by the writer in the *English Mechanic* of August 3, but the former seems to represent the peculiarities exhibited by the meteor better than the one first adopted. The meteor of July 23, 11^h 13^m, was from a radiant quite distinct to that of the Major Perseids. The last two objects represent the well-known shower of July Aquarids.

Bishopston, Bristol, W. F. DENNING.
1900, Aug. 4.

Meteoric Shower near β–γ Piscium.

WHEN sitting in my observing-chair on August 19 at 10^h 36^m, watching a beautifully clear sky for meteors in the N.E. quarter, I suddenly noticed several flashes like the play of summer lightning. I quickly turned my head towards south, but saw nothing, and then jumping up and looking to N.W. I observed the final sparks of a magnificent meteor as it disappeared at a point a few degrees below the line joining Arcturus and Cor Caroli, or at 198° +26°. This was all I saw of the object. I could not even hazard a guess as to its direction of flight.

Two days later brought me a welcome letter from the Rev. T. E. R. Phillips, of Yeovil, in which he said :—"On August 19, when engaged in observing Comet Borrelly-Brooks, the sky became suddenly and brilliantly illuminated by what I thought to be a bright flash of lightning in the west. On turning round I saw a magnificent fireball dropping rather slowly from just beneath Cor Caroli towards the horizon. For a fraction of a second it disappeared, probably behind a narrow streak of cloud, to reappear for a second or so before final extinction. The meteor seemed fully three times as bright as Venus, and the colour was white with a tinge of blue. Unfortunately, having my back to the object, I missed the beginning, but the path, so far as I observed it, was from about 191° +37° to 182½° +27°. I should say the meteor probably first appeared about halfway between η Ursæ Majoris and Cor Caroli. Time 10^h 36½^m."

Mr. Phillips's path, carried far back, crosses the point 346° +1°, from which I had seen several conspicuous meteors on the same and preceding nights, and as it forms the focus of one of the best of our minor showers in Aug.–Sept. there can be little doubt that it was also the radiant-point of the fireball. I make the heights from 56 to 29 miles over St. George's Channel and the E. coast of Ireland, and the object disappeared over Carlow. Observed path 85 miles, but the visible course was probably much longer, the meteor being only caught after the first outburst had taken place. The velocity cannot be accurately derived, but I estimated that fully five seconds elapsed between the first flash and disappearance of the object. As the meteor fell it must have blazed up several times, for I noticed a succession of flashes, probably five or six, before glimpsing the object itself at its final disruption.

As seen from the E. coast of Ireland the meteor must have been exceedingly brilliant, for Mr. Phillips estimated it as three times brighter than Venus, though he was more than 200 miles distant from the object during the latter portion of its flight.

Mr. Monck has sent me a cutting from the *Irish Times* (Dublin) of August 22, in which the fireball is described by an anonymous writer as follows:—"On Sunday night, as I was walking home with a friend, we were both surprised by seeing everything lit up with a very bright light. On looking towards the heavens we saw a large meteor shoot across in a southerly direction, leaving a long tail of fire. After about one second's duration it seemed to burst and was covered with a lurid white flame. Time $10^h\ 15^m$ [=G.M.T. $10^h\ 40^m$]."

The shower from which the meteor was directed is one which I have often seen, and it supplied the brilliant fireball of 1875 Sept. 14, for which Lieut.-Col. Tupman found a radiant at 348° ±0°. I have recorded meteors from the same point in June and July, but its principal activity appears to be manifested in August and September. It forms No. cclxviii. of the 'General Catalogue of Radiants,' with a mean position at 345°·8 +0°·1 (21 radiants). Observations at Bristol indicate the centre as follows:—

	°	°	
1899 July 31–Aug. 16	345	±0	9 meteors.
1893 Aug. 13–16	347	±0	6 ,,
1900 Aug. 15–24	346	+1	12 ,,
1885 Aug. 16–20	345	±0	7 ,,
1879 Aug. 21–23	350	±0	10 ,,
1900 Aug. 19............	346	+1	F. P.
1886 Aug. 24–Sept. 7	346	+1	5 meteors.
1885 Sept. 1–4	346	±0	9 ,,
1899 Sept. 8	347	+3	F. P.
1876–89 Sept. 15–20	346	±0	10 meteors.
1885 Sept. 17	345	±0	4 ,,
1898 Sept. 17	343	±0	S. S. P.
1886, &c. Sept. 20–Oct. 4..	347	±0	5 meteors.

The abbreviations "F. P." and "S. S. P." are radiants from the real paths of fireballs or shooting stars.

A splendid meteor was seen passing through Hercules, on Aug. 13, $10^h\ 33^m$, by several observers in the eastern counties, which was probably directed from this radiant. And it is possible that the aerolitic fragments which fell in the provinces of Jaen, Cordova, and Granada on about Aug. 23 may have been due to the descent of another large meteor belonging to this system. The peasants in the districts alluded to heard a series of thunder-claps, and then twelve pieces of an aerolite fell. One fragment, a pound in weight, was hexagonal in shape and had a grey surface.

There is another beautifully defined equatorial shower 50° west of this, viz., at R.A. 296° (=No. ccxxx. of the G. C.), and its activity seems prolonged through the months April to August. But this stream from near η Aquilæ is not in evidence every year. I saw no visible sign of it during my recent observations in July and August.

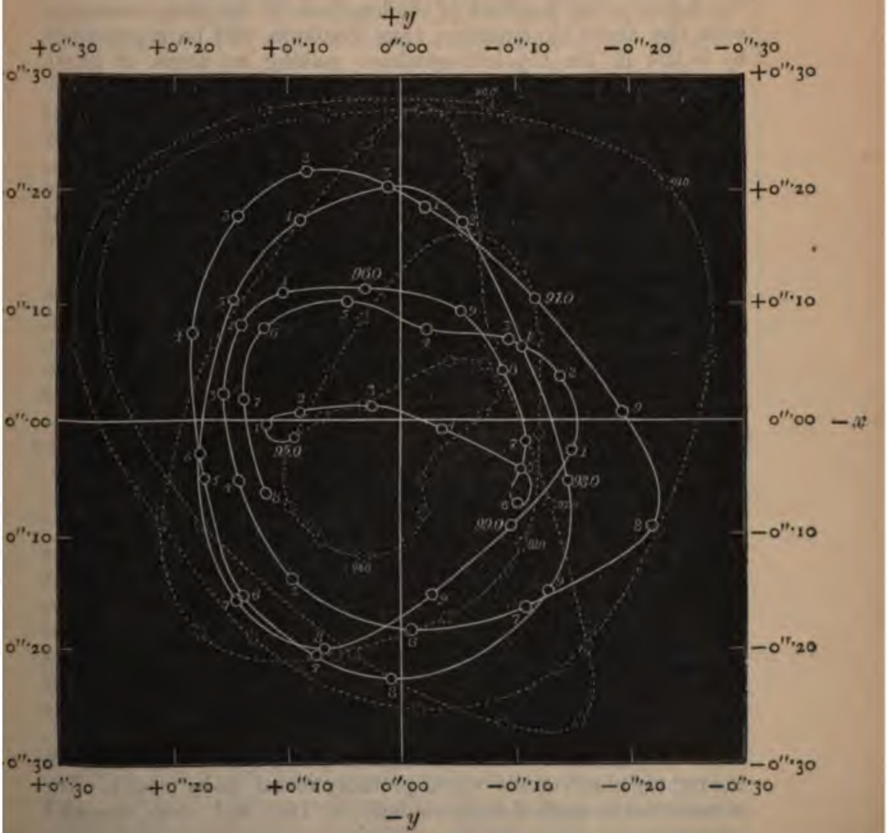
W. F. Denning.

Bishopston, Bristol,
1900, Aug. 22.

The Variation of Latitude.

It is probably well known that for several years past observations have been made by certain observatories working in co-operation to determine systematically the motion of the Earth's axis of figure about its axis of rotation, or, in more general language, the variation of latitude. Partial results have been previously given, but in March last Prof. Albrecht published * the results reduced afresh by the Central Bureau of the International Geodetic Association, the observations to the end of 1899 being included, in the convenient form of a diagram, which we take leave to reproduce.

Taking this as the ground-plan at the North Pole, the intersection of central lines being the pole of rotation, the positive *y*



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* *Ast. Nach.* 3633.

axis (upwards) representing the meridian of Greenwich, and the positive x axis (which is to the left) representing the meridian 90° East of Greenwich, the small circles show the position of the end of the axis of the Earth's figure at the times noted by the adjacent numbers representing years and tenths of years. It will be noted that the greatest distance between these poles is scarcely more that one-third of a second of arc, so that the whole motion of the pole is contained in a circle of about 70 feet diameter.

The difference of latitude of any place from its mean value at any time is given by the formula

$$\phi-\phi_0=x\sin\lambda-y\cos\lambda,$$

where ϕ_0 is the mean latitude and λ the longitude W. from Greenwich. To find this from the diagram, draw a line from the centre in the direction of the longitude of the place consistent with the above explanation: thus Poulkova will be represented by a line in the top left quadrant making an angle of about 30° with the vertical line, Washington by a line in the top right quadrant making an angle of about 15° with the horizontal line; then if from the point which represents the position of the Pole at the stated time a perpendicular be drawn to the line, the distance between the origin and the foot of this perpendicular will represent the excess of the mean latitude above the actual latitude of the place at the time, or the defect of the perpendicular falls on the line produced backward through the centre.

The dotted line shows similar results, deduced from observations in the five years 1890–1895. In our number for March 1895 we gave an exactly similar diagram * prepared by Dr. Chandler from his formulæ, with constants derived from some of the observations now treated. Comparison of these diagrams is interesting.

Meteoric Shower in July from Andromeda.

There is a very well-defined shower in July from a point about 5° N.W. of γ Andromedæ, and the position corresponds with that of the radiant of the meteors from Biela's comet in November. These July Andromedids are contemporary, and very liable to be confused, with early Perseids. On about July 21–25 the Perseid radiant is some 9° North and the observer has to be very careful in attributing the swift streak-leaving meteors to their correct centres. I make the radiant of the Andromedids at 23° +43°, from 21 meteors recorded in various years, and a careful search among my observations would probably reveal many others. The stream was in marked evidence both in 1898 and 1900, though I did not register all the paths of the meteors seen shooting from it. The following list will be sufficient to prove the existence of the shower, the radiant of which seems about 4½° in diameter:—

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* The notation of Prof. Albrecht's diagram has been altered to agree with this.

Date.	G.M.T.		Mag.	Path.				Length.	Notes.
				From		To			
	h	m		a.	δ.	a.	δ.		
1887 July 12 ...	12	17	3	340°	+64°	308°	+66°	13°	v. R. K.
1900 15 ...	11	10	4	357	+76	334	+80	6	R. K.
1898 16 ...	12	12	3	348	+53	339	+53½	5½	R. K.
16 ...	12	42	4	8	+66	0	+71	6	M. K.
17 ...	12	42	4	25½	+54½	27	+59	5	R. K.
1887 18 ...	13	15	4	12	+29	7	+22	8	R.
1878 21 ..	12	6	♃	229	+72	212	+34	39	R. K.
1900 22 ...	11	10	5	336	+69	295	+70½	14	R. K.
1876 24 ...	10	39	1	268	+79	231	+67	15	R. K.
24 ...	10	44	1	253	+79	225	+67	14	R. K.
1900 24 ...	11	11	4	85	+70	127	+67	15	R. K.
24 ...	12	18	4	23	+43	stationary.		...	K 5 secs.
24 ...	12	56	5	10	+51	2	+55	6	v. R. K.
24 ...	12	59	4	11	+12	9	+ 4	9	R. K.
1878 27 ...	11	26	3	23	+37	24	+34	3	R. K.
1877 28 ...	12	13	2	53	+62	74	+67	10	R. K.
1878 30 ...	12	17	1	352	+60½	327	+63	12	R. K.
31 ...	11	10	2	5	+45½	356	+47	7	R. K.
31 ...	13	39	3	47	+34	57	+30	9	R. K.
Aug. 1 ...	11	17	4	17	+37	15½	+33	4	R. K.
1 ...	11	44	4	31	+25	34	+20	5½	R. K.

Abbreviations:—R., rapid; K., streak.

I have seen radiants in nearly similar positions in August and October as under:—

1886 August	4–10	....	26°	+42°	6 meteors.
1877 "	12–16	....	22	+46	7 "
1884 "	19–21	....	25	+42	5 "
1879 "	21–23	....	24	+42	9 "
1887 October	14–15	....	25	+44	10 "

At the middle of October the meteors are small, slow, and short. In 1887 the radiant was 4° in diameter. I do not appear to have seen a shower from this direction in September, though I strongly suspected a radiant at 24° +44° from several swift streak-leaving meteors recorded in 1887 September 19.

From the observations tabulated it seems that this radiant near γ Andromedæ is continuously in play from the middle of July to the fourth week in August. Several other observers have seen it, and their determinations are given in my *General Catalogue* *of Radiants*, No. XXI. (Memoirs, vol. liii.), but I have no doubt this stream has often escaped recognition owing to the proximity and strength of the Perseids.

As this particular shower is liable to complication with that formed by early Perseids so there are others which are sometimes confused with late Perseids. In the third week of August there is a well-defined radiant in Camelopardalis at 61° +59°, which is only a few degrees E. of the place of the Perseid radiant. There are other showers of swift, streak-leaving meteors at 60° +49°, 70° +65°, and 71° +51°. A close observer at the Perseid epoch will in time detect these various outlying systems, but at first they are very likely to give erroneous impressions.

This year during the fine weather and great heat which prevailed during the last half of July observers were very successful in recording a number of early Perseids and determining the place of the radiant.

Bishopston, Bristol,
1900, July 27.

W. F. DENNING.

CORRESPONDENCE

To the Editors of 'The Observatory.'

Alexander Aubert.

GENTLEMEN,—

This astronomer of the last century was born at Austin Friars on the 11th of May, 1730, but educated abroad, and whilst at school in Geneva had his attention first directed to astronomy by the appearance of the great comet of 1744. He returned to London in 1751, and shortly afterwards entered into business in partnership with his father, becoming also a Director of the London Assurance Company. The first astronomical observation he is recorded to have made is that of the ingress of Venus on the Sun's disk at her transit on June 3, 1769, which he observed at Austin Friars with a Cassegrain reflector, by Short, of 2 feet focal length and power of about 110. He was elected a Fellow of the Royal Society in 1772. It was probably about that time that he established his observatory at Loampit Hill, near Deptford, for in his paper on a new method of finding time by equal altitudes, which was read before the Royal Society on November 9, 1775, and is printed in vol. lxvi. part 1, no. iv., of the *Philosophical Transactions*, he refers to observations of γ Draconis made there on the 12th of July, 1774, and gives the position of his station as latitude 51° 28' 2" North, and the longitude 2ˢ West of Greenwich. In vol. lxxiv. part 1, no. 2, of the *Phil. Trans.* is an account by him of two remarkable meteors observed on the 18th of August and the 4th of October, 1783. The former (which was seen by [illegible] many other persons, and is characterised by Archdeacon of York, who observed it at Hartlepool, as "this awful appeared a little before half-past 9 o'clock in the evening, and was noticed by Aubert whilst passing near Lewisham Bridge on his way home to his observatory at Loampit Hill; the latter came into view about a quarter before 7 o'clock in the evening, and was seen by him whilst on horseback in Blackman Street, Southwark.

Aubert observed the transit of Mercury on the 4th of May, 1786, at Loampit Hill (*Phil. Trans.* vol. lxxvii. p. 47). In 1788 he removed his observatory to Highbury House, Islington, but though it was well equipped with instruments, we do not meet with any results of observations made by him there. This may probably be accounted for by the absorbing nature of his other occupations. He was a great friend of Smeaton, the engineer, and was appointed Chairman of the Board of Trustees for the completion of Ramsgate Harbour. Political matters soon became very engrossing, and in 1797 Aubert organized and was made Lieutenant-Colonel of the "Loyal Islington Volunteers." He died on the 19th of October, 1805 (two days before the battle of Trafalgar), whilst on a visit to a friend near St. Asaph. After his death his library and instruments were sold and dispersed.

An account of Aubert, with portrait, is given in the 'European Magazine' for November 1798 (vol. xxxiv. p. 291) and a view of his house at Islington in the number for August 1799 (vol. xxxvi. p. 79). The revolving dome was removed a few years later to Dr. Pearson's Observatory at South Kilworth, in Leicestershire.

Yours faithfully,
W. T. LYNN.

Blackheath, 1900, Aug. 6.

The Eclipse of Josephus.

GENTLEMEN,—

Records of early eclipses are no doubt useful in astronomy when the date is fixed by the record, because in such cases we can test the accuracy of our present tables; but when the date is not fixed by the record and we have to endeavour to ascertain it by computing backwards, the astronomical value of the record is *nil.* It may have a historical value if the accuracy of our present tables may be relied on for the last 2000 years, but it can throw no light on the accuracy or otherwise of these tables.

For this reason I consider the eclipse of Josephus of no astronomical value. In the first place, Josephus was not born for many years afterwards, and it is not likely that any careful astronomical records were available in his time. Christ is supposed to have been 33 years of age when crucified under Pontius Pilate. Josephus mentions that he was 26 years old in the time of the subsequent Procurator Felix, and he cites no authority for the eclipse. He mentions in the same paragraph that one Joseph had acted as High priest for a day (the day of a fast) and that Herod removed Matthias from the High Priesthood, and then adds that "there was an eclipse of the Moon on this very night." Was this the night of the day on which Joseph officiated or on which Matthias was deprived? Indeed from the context it might mean the night on which another Matthias was burned alive. Even if the date of Herod's death were clearly ascertained, I doubt if the date of this eclipse could be fixed within five years, and it may have taken place on a day altogether different from those computed by our present tables.

Can any of your readers give me the date of the earliest eclipse *which is fixed by the record itself* without this process of computing backwards, which seems to me to be really begging the question?

Truly yours,

Dublin, 1900, June 5. W. H. S. Monck.

Appulse of Jupiter to β Scorpii.

Gentlemen,—

Among the noteworthy objects for the amateur star-gazer in the early half of last month may be reckoned the approach of Jupiter to this star. Through an ordinary opera-glass the two resembled a fine double star with beautifully contrasted colours. Conjunctions of Jupiter with β Scorpii are of course nothing new. They must occur almost every twelve years, only sometimes the objects are much closer than on other occasions. Some of us may remember the diagram in the 'Astronomical Register' for April 1876, by Mr. J. Birmingham, of Millbank, Tuam. It represents Jupiter and β Scorpii as seen there on February 28 at $19^h\ 25^m$ Greenwich M.T. The cross-lines show the micrometer-wires, and it seems pretty certain that contact took place. The weather unfortunately prevented observation at the actual time of conjunction.

The Chinese could not fail to notice the passage of the planet by this star. In their observations, found by Gaubil, and published in the 'Connaissance des Tems' for 1810, we have the following, under a date corresponding to

A.D. 73. "12 Feb. Jupiter tres-pies du β du Scorpion.

"16 Feb. On vit de l'etoile amparavant la lumière de le planète empêchait de la voie."

Also we are told that on April 17, 512, Jupiter occulted β Scorpii, and that on Feb. 6, 761, on May 4, 773, and on April 17, 1283, Jupiter occulted "Fang," that is the curve of stars comprising β, δ, π, Scorpionis, though which of the three is not stated.

Faithfully yours,

Melplash Vicarage, Bridport,
1900, Aug. 6. S. J. Johnson.

Annular Eclipses of A.D. 1310 *and* 1263.

Gentlemen,—

The Rev. J. T. Fowler, of Durham, called my attention, last year, to the fact that the solar eclipse of January 31, 1310, was observed at Durham Abbey. Under that date the Cellarer puts down as a memorandum, in his weekly account, 1310 "ultimo die Januar, quo die eclips. solis," and in the margin "eclipsis solis contigit isto die."

In the Durham Acct. Rolls (Surtees Society), i. 8, the following note is appended: "Mr. Percy J. Hammond" (mathematical lecturer in the University of Durham) "has ascertained that the eclipse of January 31st A.D. 1310 was an annular one. According to his calculations, the time of maximum obscuration at Durham was $1^h\ 7^m$ P.M., and the Moon would then have covered nearly seven-eighths of the Sun's disc, leaving a small crescent visible on the upper and western side of the dark face of the Moon. The eclipse would thus present a striking though not annular appearance at Durham, and a considerable amount of darkness would be caused."

A calculation from the approximate tables in the 'Encyclopædia Britannica,' 8th ed., leads me to infer that this eclipse may have been annular in the south-east corner of England; the maximum at London about a quarter past one. Oppolzer's maps would put the annular track far south of this; but they are not much to be regarded. as the said work puts the central line in the previous eclipse of Aug. 5, 1263, far into Norway and Sweden, and we have ample evidence that it crossed the Orkneys. When King Haco, sailing from Belgium, put in at Ronaldsvo, in Orkney, at this time, we are informed that "the Sun appeared as a thin bright ring"—an evil omen, it may be said, for the King, since he was not long afterwards defeated. This is one of the best authenticated annular eclipses of mediæval times. (It may be remarked that 21 years hence, viz. on April 8, 1921, the Sun again will appear as a "thin bright ring" across the Orkneys, including, probably, a little slice of the north-west part of Sutherlandshire, the only case of an annular eclipse in any part of Great Britain until July of the year 2093.)

Melplash Vicarage, Bridport,
1900, Aug. 6.

Faithfully yours,
S. J. JOHNSON.

Magnitude of B.A.C. 245.

GENTLEMEN,—

Some time ago, in looking through Chambers's Catalogue of Naked-eye Stars in vol. iii. of his 'Astronomy,' my attention was attracted by a star of the 5th mag. of which the Harvard reference-number was not given, although the star was within the limits of the H.P. The star's name and position, as given by Chambers, are:—B.A.C. 245 Cassiopeiæ, R.A. $0^h\ 48^m{\cdot}3$, Decl. $+48°\ 2'$, mag. 5.

The mag. is evidently that of the B.A.C. and an eye-estimation, as the star does not occur at all in the H.P., and, as the Chambers does not give any mag. in the column devoted to Oxford measures, I presume the star does not occur in the 'Uranometria Nova Oxoniensis' either.

On looking up the star on the maps, I found that though it is shown as a 5th mag. star on Cottam's 'Chart of the Constellation Cassiopeia,' and also on the chart of that constellation in Proctor's 'New Star Atlas,' it is absent from Klein's 'Atlas.' The star forms a quadrilateral with ν, ξ, and o Cassiopeiæ.

About 10 P.M. on August 17, I looked for the star with the opera-glass, and found it was slightly inferior in magnitude to H.P. 164 (D.M. +51°·220), the mag. of which is 6·40, according to the H.P. At the time of observation the comparison-star was nearly at the same altitude as the star in question.

Is this discrepancy due to some mistake in the B.A.C., or is the star variable? S. B. GAYTHORPE.

Barrow-in-Furness.

Mr. W. Bryant kindly supplies the following note:—

The star B.A.C. 245 = Groombridge 171 = Lalande 1511 = Lalande (Fedorenko) 140 = Oeltz. Arg. (N.) 863 = Radcliffe 247, &c. &c.

Groombridge gives mag. 5 (1810), possibly copied by Fedorenko, and certainly by B.A.C. and Greenwich Catalogues.

Lalande, however, gives 6½ mag. (1797·8).
Argelander (1842) 7 (probably should be brighter).
Radcliffe (1845) 6·2.
Bonn Durchmusterung, 1855, vol. v., 6·5.
Gesellschaft Cat. (Bonn, 1875), 6·3.

So in all probability the 5 mag. is an error, the star being about Mr. Gaythorpe's estimate of 6½ mag., and not variable.

Royal Observatory, Aug. 21, W. W. B.

New Forms of Telescopes and other Optical Instruments.

GENTLEMEN,—

There have recently appeared in the newspaper and scientific press communications dealing with a new form of telescope, the fundamental principle of which consists in attaining the achromatism of a large object-glass by the interposition of a small concavo-convex lens, silvered on the back—the curvatures of the small correcting negative lens, with its internal reflecting surface, being so proportioned as to cause its negative chromatism to correct the positive chromatism of the object-glass more completely than has yet been attained, at the same time at a much less cost and with a shorter tube.

It has recently been announced that Professor Schupmann, of the Technische Hoch Schule at Aix-la-Chapelle, in Prussia, on July 30, 1897, applied for and obtained in the United States (under Letters Patent No. 620978, dated March 14th, 1899) a patent on such an instrument as is above described, under the name of "Medial-Fernohr." Among other things it is claimed that one of the advantages of the telescope is that single crown-glass lenses alone may be used. Prof. Schupmann, it is also announced, has published a book on the subject.

Under the circumstances, and in defence of the interests of two of its members, Messrs. Z. M. and J. R. Collins, the Toronto Astronomical Society thinks it right to intervene for the purpose of laying before your readers certain facts not hitherto published, and which may tend to place the alleged new invention in a different light.

In 1893 the Messrs. Collins constructed a Telemeter of their own design in which a modified form of dialyte objective was used for the purpose of shortening the tube and amplifying the focus. As a further result of their studies they came to the conclusion that certain principles employed in their Telemeter might be advantageously used in a telescope combining some of the properties of a refractor and a reflector. In the summer of 1896 they completed and exhibited to a few friends and a number of members of this Society an instrument of 4 inches aperture, 2 feet in length, and 4 feet focus, which they called the "Monoplane Achromatic Telescope." This instrument performed admirably, photographs being taken with it. On the suggestion of the Messrs. Collins, confidential communications were, in the spring of 1897, addressed to Lord Kelvin, and to Prof. J. A. Brashear, of Alleghany, Pa., and Dr. H. C. Vogel, Potsdam, Prussia. To these gentlemen were also submitted drawings as well as descriptions of the fundamentals of the Monoplane combination. On the data before him Lord Kelvin declined, however, to express a conclusive opinion; Prof. Brashear, while not committing himself to the principle involved, suggested that an 8-inch telescope of high quality should be constructed and tested. In his first communication Dr. Vogel, who stated that he had shown the invention to his assistants, unsparingly condemned the telescope. But after a letter explaining to him an error in the figures first supplied him, together with enlarged explanations, Dr. Vogel then said the combination appeared to him in an entirely new light, and with the figures then before him he found the correction for chromatism "to be more complete, indeed, than is the case with the ordinary achromatic objective."

As a result the construction of an instrument of 8 inches aperture was proceeded with, the late President, Mr. Arthur Harvey, F.R.S.C., and Mr. John M. Martin joining them with a view to furthering its completion. The development, however, of an important feature of the combination (not mentioned in the communications referred to and not included in Prof. Schupmann's patent specifications) delayed the construction of the second instrument, and it was not until this was nearly completed that Prof. Schupmann's patent proceedings became known.

The point which the Society wishes to make is that, so long ago as the early part of 1896, a telescope embodying the essential features of that described by Prof. Schupmann was constructed and tested in Toronto. In each of two annual addresses to the Society delivered in January in each of the years 1896 and 1897, Mr. John A. Paterson, M.A., President, referred to the instrument and claimed for the Society the credit of having members sufficiently skilled in optics to produce a combination of lenses composed of one kind of glass, which could be used for telescopes, microscopes, and cameras, greatly cheapening the cost and reducing the size of these instruments.

In the address of 1896 he says:—"The Collins Brothers have proved their ability in the figuring and polishing of parabolic mirrors for reflecting telescopes, and recently they have invented a telescope of an entirely new design, which will attain a maximum result at a minimum cost, measuring only half the length of the ordinary design, bearing at the same time the same magnifying-power and possessing the virtue of achromatism by the use of two lenses, both of crown glass. By changing the relative position of the chief lenses the achromatism can be under-corrected or over-corrected. A short achromatic telescope with two lenses of the same material sounds like an impossibility."

In the address of 1897 he says:—"Two of our most earnest members, the Collins Brothers, are still developing their new Monoplane Achromatic Telescope. It is one of 8 inches aperture, having a new form of objective of one kind of glass. Mounted equatorially it delivers an image of a celestial object in front of the eyepiece in dimensions equivalent to a length of 40 feet, although the extreme length of the instrument complete is but 4 feet. It is able to correct for spherical aberration as well as chromatism and also for the 'Schaeberle' aberration that must necessarily exist in the ordinary refractor of great angular aperture. Dr. H. C. Vogel, of the Astrophysical Observatory, Potsdam, after a mathematical analysis of the correction for achromatism, says, 'a workable objective is now shown which, as the reasoning proves, unites in an excellent manner the rays from red to violet, better indeed than is the case with an objective of the usual construction.' It may be added that the same principle may be applied to the microscope."

Upon this subject the Society is preparing a special report, which will include copies of the correspondence which passed between the Society and the authorities referred to.

In justice to the Messrs. Collins, I am to ask you to be so good as to find room in your valuable magazine for this communication.

W. B. Musson,
Sec. Toronto Astr. Soc.

OBSERVATORIES.

Windsor, N. S. Wales.—The observing weather was very favourable during 1899, so that Mr. Tebbutt has a large amount of work to record. Clock-error was determined on 195 nights with the transit-instrument, the details and results of which are given in the proprietor's Report for the year: a remarkable occurrence in this section is that the level-error changed by more than 4" in the course of 9 days, which Mr. Tebbutt thinks was due to a heavy rainfall, as 5 inches of rain fell between 9 A.M. on August 17 and the same hour on August 25, and the level was found thus altered on the evening of the latter day.

Seventy-six phases of occultations, various micrometer observations of minor planets and comets, were made, as well as the meteorological observations; but Mr. Tebbutt finds that advancing years compel him to forego such vigorous work, and he intends for the future to devote himself solely to double-star observations, as the reduction of these is comparatively easy. He seems to find it difficult to get efficient computing help in the Colony.

PUBLICATIONS.

Dr. Cohn has determined the proper motions of 192 stars used in the International Geodetic Service, and these, with the N.P.D.'s for 1900, are published by the Central Bureau as "Neue Folge der Veröffentlichungen, No. 2"*. The stars fall into two classes, viz., those used for latitude determination (between +15° and +62° Dec.), and those used for researches on refraction (between −18° and −25° Dec.). There are a few between +76° and +83°.

Dr. Cohn gives all the catalogue-places for each star and the precession used in reduction, which adds to the value of the work for reference.

NOTES.

Comet Notes.—Comet *b* 1900 (Borrelly-Brooks) was a very conspicuous object early in August, and a large number of observations was secured. A tail about half a degree in length was visible, and the nucleus was of the 8th magnitude or brighter. The comet moved into very high north declination, but is now going south again. The following elements are by Mr. Perrine from observations on July 25, 30, Aug. 4 (*Ast. Nach.* 3656):—

T	1900 Aug. 3·21 G.M.T.	
ω	12° 26′	1900·0.
☊	328 1	
i	62 31	
$\log q$	0·0064	

Ephemeris for Greenwich midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
Sept. 3....	13 2 40	80 47	Sept. 15 ..	14 0 16	73 59
7 ...	13 30 16	78 12	19 ..	14 8 52	72 20
11....	13 47 28	75 58	23 ..	14 16 48	70 50

The brightness is rapidly diminishing, and on Sept. 23 will have sunk to one-tenth of that at discovery, so that the comet will scarcely be followed for longer. A. C. D. C.

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* 'Ableitung der Declination und Eigenbewegungen der Sterne für den Internationalen Breitendienst.' Von Dr. Fritz Cohn. Berlin, 1900: Georg Reimer.

MINOR PLANET NOTES.—M. Loewy has published an ephemeris of (433) Eros based on Signor Millosevich's elements, corrected by recent observations. We give an abridgment of it (for Berlin midnight):—

	R.A.			N. Dec.			R.A.			N. Dec.	
1900.	h	m	s	°	′	1900.	h	m	s	°	′
Sept. 19....	2	39	3	40	24	Oct. 13....	2	40	53	49	13
23....	2	41	27	41	56	17....	2	37	36	50	29
27....	2	43	7	43	27	21....	2	33	7	51	38
Oct. 1....	2	44	0	44	57	25....	2	27	27	52	37
5....	2	43	58	46	26	29....	2	20	44	53	24
9....	2	42	58	47	51	Nov. 2....	2	13	11	53	58

The planet is of about 10½ magnitude at the commencement of this ephemeris, and about 9½ at the end.

M. Loewy has also published a list of 319 stars near the path of Eros, and suitable for comparison stars. As many observatories as possible are requested to co-operate in obtaining accurate places of these stars by meridian instruments; with a view to homogeneity of results, they should be referred to Newcomb's fundamental catalogue as a basis. A. C. D. C.

THE OPPOSITION OF EROS.—The Committee who are organising the observation of this small planet during the coming months are losing no time in beginning their task, for the energetic Bureau, consisting of MM. Loewy, Bakhuyzen, and Trépied, have already issued two circulars. The first and second Resolutions of the Committee are as follows:—

I. It is desirable that the determination of the position of the planet Eros should be made by micrometric, heliometric, and photographic measures:

(*a*) By means of observations of the planet at the same observatory east and west of the meridian.

(*b*) By the co-operation of observatories of Europe and North America.

(*c*) By the co-operation of observatories of the northern and southern hemisphere.

II. During the period of observation for parallax, the diurnal movement of the planet Eros should be determined as exactly as possible by means of micrometer, heliometer, and photographic measures.

There are other resolutions, but the above give the scheme of operations. Paragraph (*a*) of Resolution
by the condition that the observations east an

meridian are to be made in the greatest hour-angle possible, provided that the planet has an altitude of at least 20°.

The necessity for the determination of the motion mentioned in Resolution II. will be seen by referring to the diagrams given in our last number. If observations were made simultaneously at two places, so situated that the displacement would be at right angles to the short trails formed by the planet on photographs, it would only be necessary to measure the perpendicular distance between the trails (supposing them on one plate), and this would be sufficient data for the determination of the parallax; but if this ideal condition is not attained, it will be necessary to know the exact angle that the displacement makes with the trail—that is, the direction of motion of the planet. It is proposed to find the planet's motion independently of parallax and all theory by observations made daily, and more than once daily, at each observatory. The suggested programme for European observatories is that observations should be made in large hour-angles east and west, with a supplementary series of observations about 2 hours before the second or western series. In North American observatories a supplementary series should be made about 2 hours after the first or eastern series. By working in this way it will be possible to obtain, by a kind of interpolation, the equivalent of *simultaneous* observations in Europe and America as well as to find the motion of the planet.

The Committee also point out that it will be advisable to begin at once to make experimental observations. Especially with regard to photography, they suggest that experiments should be made in measuring the positions of small trails.

The Crossley Reflector.—A very readable description of this instrument as it now is and the work done with it at Lick appeared in the *Astrophysical Journal* for June last, from the pen of Prof. Keeler. There are many little hints contained in it, instructive to a photographic observer. The following seems especially worth quoting, although, as the fact involved seems to have been the result of accident, it might be difficult for everyone to follow up the hint: in the guiding eyepiece there are spider-webs, and the tension of one of these is such that it begins to slacken when the temperature falls to within about 2° of the dew-point. The web thus forms an hygrometer, and the observer knows that when he sees this thread slacken it is time to cover the mirrors.

Prof. Keeler says that the large mirror, the figure of which is excellent, was made by Mr. Calver. It seems right to point out that, though he made the mirror originally, its present form is due to Sir Howard Grubb, for, having by some means got into a bad condition after ten years' wear, it was refigured at Dublin in the year 1890 or 1891.

SPIRAL NEBULÆ.—The Crossley reflector is used now chiefly to photograph nebulæ, and some copies of these photographs have already been published; but Prof. Keeler evidently thinks that the interest of the work is not in the beauty of the pictures, but in conclusions to be drawn from them, which he summarizes as follows:—

"1. Many thousands of unrecorded nebulæ exist in the sky. A conservative estimate places the number within reach of the Crossley reflector at about 120,000. The number of nebulæ in our catalogues is but a small fraction of this.

"2. These nebulæ exhibit all gradations of apparent size, from the great nebulæ in *Andromeda* down to an object which is hardly distinguishable from a faint star-disk.

"3. Most of these nebulæ have a spiral structure.

"To these conclusions I may add another, of more restricted significance, though the evidence in favour of it is not yet complete. Among the objects which have been photographed with the Crossley telescope are most of the 'double' nebulæ figured in Sir John Herschel's catalogue (*Phil. Trans.* 1833, plate xv.). The actual nebulæ, as photographed, have almost no resemblance to the figures. They are, in fact, spirals sometimes of very beautiful and complex structure; and, in any one of the nebulæ, the secondary nucleus of Herschel's figure is either a part of the spiral approaching the main nucleus in brightness, or it cannot be identified with any real part of the object. The significance of this somewhat destructive conclusion lies in the fact that these figures of Herschel have sometimes been regarded as furnishing analogies for the figures which Poincaré has deduced, from theoretical considerations, as being among the possible forms assumed by a rotating fluid mass; in other words, they have been regarded as illustrating an early stage in the development of double-star systems. The actual conditions of motion in these particular nebulæ, as indicated by the photographs, are obviously very much more complicated than those considered in the theoretical discussion.

"While I must leave to others an estimate of the importance of these conclusions, it seems to me that they have a very direct bearing on many, if not all, questions concerning the cosmogony. If, for example, the spiral is the form normally assumed by a contracting nebulous mass, the idea at once suggests itself that the solar system has been evolved from a spiral nebula, while the photographs show that the spiral nebula is not, as a rule, characterized by the simplicity attributed to the contracting mass in the nebular hypothesis."

IN *A. N.* 3652 Dr. Kreutz shows that the double star β 107 is B.D. $+62^\circ \cdot 94$, not B.D. $+62^\circ \cdot 93$. This smooths somewhat the discrepancies pointed out by Burnham in the Yerkes volume.

Speaking of the Yerkes volume recalls Prof. Hussey's "Notes on the Progress of Double-star Astronomy" in No. 74 of the 'Publications of the Astronomical Society of the Pacific.' These notes give, in a concise manner, an idea of the work in this department up to and including Dembowski's Catalogues, and naturally lead to a fuller appreciation of Burnham's early work, and enable Prof. Hussey to pay his tribute of admiration to vol. i. 'Yerkes Publications.'

To this work of Burnham and to the Southern Catalogue published by Mr. Innes, more than half the notes are devoted. By misplaced modesty the recent steady work at Lick by Professors Hussey and Aitken receives but scanty notice.

The same No. 74 contains an orbit of τ Cygni by Prof. Aitken. The observations are few and show large discrepancies *inter se*, making an orbit difficult. Dr. See has recently determined an orbit of this pair, and gives 57·25 years as period. Aitken gives 45·1.

Some new and interesting doubles by Aitken and Perrine are also to be found in this number.

Recent measures of Sirius:—

1899·90	145°·6	4″·53	Aitken.	4 nights.
1900·30	150 ·9	4 ·64	See.	4 ,,

Recent measures of Procyon:—

1900·06	336°·0	5″·09	Barnard.	6 nights.
1900·24	338 ·3	4 ·83	Lewis.	1 ,,
1900·30	332 ·4	4 ·60	See.	4 ,,

THE REPORT OF THE ASTROGRAPHIC CONFERENCE: CORRECTION.—It may have been inferred from the occurrence of several figures in brackets in the table of Reports of work done on the Astrographic Chart given on p. 306 of our last number that this information was to some extent approximate. It was, in fact, kindly collected for us by Prof. Turner at the Conference table, and we had no means of checking the figures. It has been pointed out to us that some injustice was done by saying that *no plates of long exposure* had been taken at the San Fernando Observatory, for the actual fact is that 539 have been taken with 3 exposures of 30 minutes each, and 125 with an exposure of 60 minutes, which is more than half the number allotted to this Observatory. All the Catalogue plates have been taken. We hope that Admiral Viniegra will pardon this mistake, and we will ask readers to make the correction in the table, lest it might seem at any future time to cast a reflection on the energetic observatory over which he has control.

ERRATUM.—In last month's number, p. 328, line 3 from bottom, *for* Prof. W. H. Pickering *read* Prof. & Mrs. E. C. Pickering.

FROM AN OXFORD NOTE-BOOK.

BURNHAM'S 'General Catalogue of Double Stars' makes a splendid first volume for the Yerkes publications. Especially would I commend to the casual reader the Introduction—a dozen pages (excluding tabular matter) in which a master of the subject sets down what is to be said about it. And there are some important things to say: double-star work has been revolutionized in the last 30 years. "Prior to 1870 all the leading double-star catalogues combined, including altogether not less than 7400 so-called double stars, contained less than 700 pairs with distances not exceeding 2″." Lately it has been realized that attention should be restricted to close pairs, for wide pairs are seldom binary. Again, 30 years ago "it seems to have been practically accepted that the field for the discovery of new pairs had been substantially worked out by the Herschels and the Struves." Since then the number of *close* pairs has been more than doubled, "and the prospect of future discoveries is as promising and encouraging as when the first star was found with the six-inch telescope." These are words, not only of wisdom, but of encouragement in no small degree.

ANOTHER Catalogue is well worthy of mention—the new Madras Catalogue of 5303 stars for epoch 1875·0. It is no light matter to deal with 25 years' observations made by another man, and Mr. Michie Smith is to be warmly congratulated on the completion of an arduous task which he has carried out most thoroughly. We who were at Sahdol for the eclipse of January 1898 admired the patience and zeal with which he built up a mountain to support one end of his long telescope; now we see that he can demolish a mountain — of arrears of work accumulated by another man. Allowing breathing-space, he will soon be in a position to commence work on his own initiative in a new observatory, and we shall await the results with the keenest interest.

THE mention of eclipses reminds me that I collected a few of the more interesting results of the last eclipse—according to the daily papers. One writer says:—

> It is too early yet to estimate the full significance of the observations; but I have reason to believe that the most valuable shadow passed at forty-six miles per minute.

I have not even yet estimated the full significance of this observation, though I have some glimmering of what was meant by "thirty-six plates being taken with an intra-mercurial apparatus" and by the following sentences:—

> The streamers seemed to be nearer the solar *equation* than in 1898.
>
> The existence of two prominences on the equator of the Sun is confirmed.

We have heard a good deal of the inaccuracy of the *Nautical Almanac* predictions, but it is alarming to read that

> the professors of the McGill University report that the calculations were two *minutes* behind.

As a general description of the eclipse it is difficult to beat the following: the anxiety of the cats is specially picturesque:—

> It then developed faster into strange effects of light, in darkening the atmosphere like a weird twilight, which soon made workmen stop their labours, and caused animals to show surprise, and caged birds to stop singing, whilst cats anxiously moved about the houses.

It is noteworthy that the workmen stopped their labours apparently before the animals showed surprise; perhaps the surprise was occasioned by the stopping work? for the workmen were, of course, not British workmen.

FROM a South Australian paper I learn that Sir Charles Todd reached the age of 74 on July 7, and is within a few months of his Diamond Jubilee in the service of the Queen! One cannot but regret that the information did not come in time to allow of our joining in good wishes on the actual day, and I hope other people who are going to be 74 and to celebrate their 60 years of service will let us know in some way beforehand. It is certainly a wonderful record, and I wish there were room to reproduce here all the interesting reminiscences of early telegraphic work in the Colony which a fortunate interviewer secured for the occasion. One little story has special claims on an Oxford Note-Book:—

> "Yes; I met with a few adventures during my solitary rides from Melbourne, and subsequently back again through the South-Eastern districts, and in various parts of the province when inspecting lines. I remember one experience, although it is by no means an adventure, which is characteristic of the class of men I used to meet in the bush. I came one night to a shepherd's hut, and enquired of the solitary occupant if I might stay there. 'Do as you like,' was the rather uninviting rejoinder. 'Then I will,' I replied; and at once hobbled my horse. We had some damper and a billy of tea, and, noticing a home-made chessboard, I suggested a game. We became quite friendly, and the lonely shepherd told me his history. He was a Master of Arts of Oxford!"

There is another paragraph which is worthy of reproduction because it affords convincing evidence of the genuineness of the interview:—

> "I'm afraid I should only weary you and others," was the response. Sir Charles, who had observed me on my bicycle, had previously kindly enquired if my machine was "tired." My survival of such a flank movement evidently convinced him that I belonged to that wiry type of Australian natives who, according to Dr. Marten, are hard to kill. At all events, I risked the weariness, and can safely recommend others to do the same.

Now that is Sir Charles "every time" as the Americans say. I have only known one man ever attempt to keep up with him as a punster, and this solitary rival has to take cayenne-pepper as a stimulant. There is so much bogus interviewing now-a-days that it is worth keeping an eye on internal evidence of this kind.

THERE is an interesting theory of the *Gegenschein* in *Ast. Journ.* No. 483, due apparently in the first instance to Gylden, but suggested quite independently by Mr. F. R. Moulton. He supposes that there are in the neighbourhood of a point 900,000 miles from the Earth, and constantly opposite to the Sun, a number of meteors describing small oscillatory orbits. The case of such oscillatory satellites occurs in Prof. Darwin's studies on Periodic Orbits, and the motion is found to be unstable. Thus no individual meteor can remain permanently in the neighbourhood of this point, but it may stay long enough to dance a few times before it is swept onwards; and its place may then be taken by another. The suggestion is full of interest as bringing within our consideration for the first time a possibly real case of motion such as recent work on Periodic Orbits has suggested, quite different from the steady-going (nearly) circular orbits of the planetary and lunar theory.

IN No. 481 of the same journal there is a discussion of the old question of the R−D discordance at Greenwich by Prof. J. R. Eastman. I fear he will not get many people, at any rate many who have had experience at first hand, to accept his conclusions. He makes no reference to several recent discussions of this discordance by the Greenwich observers themselves: have these escaped his attention?

THERE is an ancient joke about a man who could understand our determining the distances of the stars, but was puzzled how we found out their names. An inverse puzzle has been brought to my notice by Mr. Denning; it almost seems as though meteors must be able to find out the names of places on the Earth. For instance, on July 24 last, a meteor seen by six observers started from a point three miles N.E. of *Burn*ham on the Essex coast and disappeared over a point fifteen miles E. of *Burn*ham, Norfolk. It passed nearly over *Bright*lingsea, Essex, and one of its brilliant outbursts of light occurred over *Burst*on in Norfolk. Another meteor of apparently a humorous turn of mind appeared and disappeared nearly over *Quainton*, Bucks, and its earth-point was near *Odd*ington, Oxfordshire.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXIII. OCTOBER, 1900. No. 297.

Note on the Resolution of Double-Stars with a 28-inch Object-glass.

The image of a star consists of a spurious disk and diffraction-rings. We are only concerned with the disk.

It can be inferred, by Airy's method, supplemented by the method employed below, that with a 28-inch objective of which the aplanatic correction is perfect, the disks of two stars seen by light of wave-length 0·57 of a micron[*], should be seen *separate*[†] on a good night if the interval between the stars is not less than $0''\cdot4$; that if the star-interval is between $0''\cdot4$ and $0''\cdot17$ the two disks merge into a dumb-bell shaped image formed by light, some of which is capable of showing the disks separated, but that if the interval falls a little short of $0''\cdot17$, none of the light is competent to show them separated. These latter results are obtained by applying the formula which follows to an objective of 28 inches aperture.

In the second case, *i. e.* when the interval between the stars is between $0''\cdot4$ and $0''\cdot17$, the image is produced by light some of which can and some cannot produce an image consisting of two *distinct* disks. The image will accordingly be cleared by excluding

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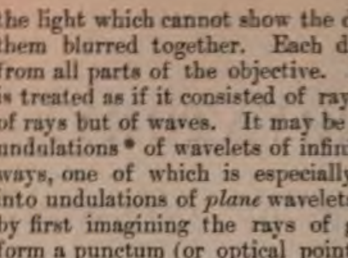
[*] I find 0·57 of a micron a good wave-length to assume when dealing with resolutions by white light—not monochromatic.

[†] In the image of a star formed by light of wave-length λ, the spurious disk extends over the space enclosed by the first dark ring, the diameter of which was shown by Airy to be

$$d = 2\cdot44\,\frac{\lambda}{A},$$

where A is the diameter of the object-glass. This disk consists of a brighter central part with fainter outlying portion. With a good objective the bright part may roughly be taken to have a diameter equal to $d/2$; but the disks of two stars are not spoken of in the text as separate or distinct so long as *any* part of the disks overlap. Accordingly, in order that they may be separate, the interval between the centres of the images of the stars must equal or exceed d.

the light which cannot show the disks as distinct, but which shows them blurred together. Each disk is produced by light coming from all parts of the objective. Now light in geometrical optics is treated as if it consisted of rays, but it does not really consist of rays but of waves. It may be resolved into the co-existence of undulations * of wavelets of infinitesimal intensity in innumerable ways, one of which is especially convenient, viz., the resolution into undulations of *plane* wavelets. These are most easily pictured by first imagining the rays of geometrical optics converging to form a punctum (or optical point) of the image. Next conceive undulations of plane wavelets all of the same wave-length and with wave-fronts perpendicular to each of these rays, and all reaching the position of the image in the same phase. Then I have elsewhere proved that one of the ways in which the actual light of wave-length λ may be resolved is into these undulations of *plane* wavelets †.



We may retain the word ray, if we remember that it has no physical existence, but is only a geometrical line, viz., that normal to the wavelets of one of the above undulations which passes through the punctum of the image we are considering. Now every pair of undulations would, if they alone were present, produce a luminous ruling in the plane of the image, and the whole image may be regarded as formed by the coexistence of all the rulings which are formed by grouping the undulations in pairs. *It is legitimate to group them in any pairs that we find most convenient, provided that in doing so we use up all the light.*

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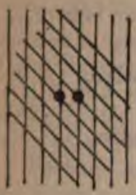
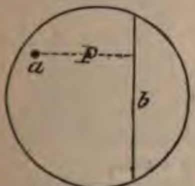
* By an undulation is to be understood a train of similar equidistant waves If the undulation is of plane waves they advance without change. If they are spherical waves, the transversal changes but the wave-length remains unaltered.

† This theorem was enunciated in the first of three papers on Microscopic Vision by the present writer in the *Phil. Mag.* for October 1896, p. 335; and a somewhat clumsy proof is there given of it. A simpler proof is given in the *Phil. Mag.* for April 1897, p. 273. But a still simpler proof may be obtained by describing a larger and smaller sphere with radii R and r round a punctum or luminous point emitting spherical waves. After these spherical waves have travelled beyond both spheres, reverse the motion. Thereupon the outgoing spherical waves become convergent spherical waves travelling inwards. According as they reach the surface of sphere R, on their inward journey, let the motion be resolved by Huygens's principle into undulations of spherical wavelets emitted inwards from the innumerable puncta of the surface of sphere R. Each of these undulations of spherical wavelets, when it reaches sphere r, becomes a train of convex wavelets crossing that sphere; and the coexistence



Let σ be the interval from centre to centre of the images of two stars, and let f be focal length of objective. Then σ/f = angular separation of the stars. Let the figure represent the two spurious disks, and the lines represent two of the aforesaid rulings. Then it can be proved that the light which can show the disks *separate* is light which can be resolved into rulings such that two of the crests of each ruling are on the centres of the two disks, as in the figures. We need only consider the case when two *consecutive* crests are on the two images.



Now the undulations which can effect this are found as follows:—Let the figure represent the objective, and let us consider the undulation consisting of those plane wavelets which are perpendicular to the ray drawn from a to the image.

Draw p *parallel to the line connecting the two disks*, and of a length such that

$$\frac{\sigma}{f}=\frac{\lambda}{p}\,[*].$$

Then the undulation whose ray comes from the point a, in conjunction with any one of the undulations whose rays come from any of the points of the perpendicular line b, will produce one of the required rulings with light of wave-length λ.

Hence if p is more than 0·355 of a metre (which is ½ the diameter of the objective), we shall gain an advantage by covering a central strip of the objective the width of which B is to be

$$B = 2p - A,$$

A being 28 inches or 0·711 metre.

---

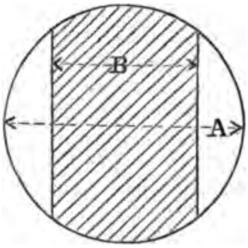
within sphere r of all these trains of convex wavelets produces the same motion within that space as would be produced by the original single undulation of converging spherical waves. The convexity of the wavelets within sphere r may be made as small as we please, by increasing the size of sphere R, and, at the limit, they become undulations of plane wavelets.

Thus it appears that a single undulation of converging spherical waves furnishes the same motion of the medium within sphere r as would result from the coexistence of innumerable undulations of plane wavelets traversing that space. Finally, by a second reversal the divergent condition of the light may be restored; moreover r may be made of any size we please.

As the spherical waves emitted by each punctum of an object or image can be resolved into undulations of plane wavelets, it follows that the whole of the light from the object or image can be so resolved.

[*] In this formula f/l is treated as unity, where l is the distance of the image from the *edge* of the objective.

Case 1.—Thus if δ be the separation of the stars, and if



$$\delta = \frac{\sigma}{f} = 1/3'' = 0{\cdot}000{,}001{,}6,$$

and if

$$\lambda = 0{\cdot}000{,}000{,}57 \text{ metre},$$

then

$$p = \frac{57}{160} = 0{\cdot}356 \text{ metre},$$

and

$$\mathrm{B} = 0{\cdot}712 \text{ metre} - 0{\cdot}711 = 1 \text{ mm.},$$

so that theoretically covering a central strip of the objective one mm. wide would gain an excessively small advantage with light of wave-length λ.

Case 2.—If $\delta = 1/4'' = 0{\cdot}000{,}001{,}2$,

and λ as before, then

$$p = \frac{57}{120} = 0{\cdot}491 = 49{\cdot}1 \text{ cm.},$$

and $\mathrm{B} = 0{\cdot}982 - 0{\cdot}711 = 0{\cdot}271 \text{ metre} = 27{\cdot}1 \text{ cm.}$,

which is therefore the width to be given to obstruction B in this case.

Case 3.—If $\delta = 1/5'' = 0{\cdot}000{,}000{,}97$,

then $p = \frac{57}{97} = 0{\cdot}5876 \text{ metre}$;

$$\therefore \quad \mathrm{B} = 1{\cdot}75 - 0{\cdot}711 = 0{\cdot}464 \text{ m.} = 46{\cdot}4 \text{ cm.},$$

so that in this case it would be necessary to cover a strip of the objective nearly 46½ cm. wide in order to clear the image.

Case 4.—If δ were

$$0''{\cdot}18 = 0{\cdot}000{,}000{,}873,$$

then $p = \frac{\lambda}{\delta} = 0{\cdot}653 \text{ metre}$;

$$\therefore \quad \mathrm{B} = 2p - \mathrm{A} = 1{\cdot}306 - 0{\cdot}711 = 0{\cdot}595 \text{ metre},$$

or 59½ cm., which is the width of the covering strip to be used in this case.

Case 5.— $\delta = 0''{\cdot}17 = 0{\cdot}000{,}000{,}824{,}5$,

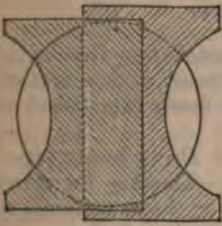
$$\therefore \quad p = 0{\cdot}691 \text{ metre},$$

and $\mathrm{B} = 1{\cdot}382 - 0{\cdot}711 = 0{\cdot}671 = 67{\cdot}1 \text{ cm.}$

This is the width of the obstruction in this case.

This would leave a luminous strip only 2 cm. wide at each side. and goes probably as far as it will be practicable to go in the resolution of double stars with a 28-inch objective which is aplanatic up to the margin, unless we exclude the red and orange light, in which case λ may probably be made as small as 0·52 μ, and if so a complete separation of the disks can be effected by a 28-inch objective upon a double star whose constituents are only 0″·155 asunder.

Perhaps the simplest plan would be to have a light frame made to go over the objective with two sliding panels, so that the width of the obstruction can be adjusted, the whole frame being mounted to turn into any position over the object-glass, so as to bring the strip B perpendicular to the line joining the stars.



The image may be further cleared by curving the outer edges of the sliding panels as in the annexed figure, the curvature being the same as of the outline of the object-glass. This form of shutter excludes all light which is incapable of furnishing *cipher* illumination at the contact of the spurious disks of the two stars, and it is obviously an advantage to secure this result.

8 Upper Hornsey Rise, N.,
1900, June 30.

G. Johnstone Stoney.

Postscript.—*How to deal with objects of visible size.*—A circular stop which blocks out all but an outer annulus of the objective is the best for seeing minute detail on the Sun, Moon, and Mars.

If the outstanding annulus is ½ a centimetre wide, then as much light passes as would be transmitted by an unobstructed objective of 3·3 inches aperture. This would probably answer for examining the Sun, and would probably bring more detail into view than has ever yet been seen. The stop should be made of highly-polished metal * to prevent its becoming quickly heated, and the whole instrument should be screened from the Sun till the instant of making the observation. A windy day would probably be the best for the observation.

If the annulus left uncovered by the stop is—		The quantity of light that passes is the same as would be transmitted by an objective of—
1 cm. wide	then	4·7 inches aperture.
2 cm. „		6·6 „ „
3 cm. „		8 „ „
4 cm. „		9·3 „ „
5 cm. „		10·3 „ „

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* Brass would probably be a good material, as it is said when polished to reflect heat copiously.

The last of these (the annulus about 2 inches wide, left by a circular stop 24 inches in diameter) will probably be the best for examining the Moon and Mars.

By excluding all the central light we protect the image of minute details upon such objects from being swamped by the presence of light, which cannot show the minute details and which would blur it together; and at the same time we retain enough of the light which furnishes the coarser details.—G. J. S.

The Geminid Meteor-Shower.

The shooting-stars that radiate from the constellation Gemini in the first half of December are of particular interest, as the question of the shifting of their centre has yet to be firmly established or disproved.

The maximum of the shower is due about December 10–13—unfortunately in 1900 a period of moonlight. This has for many years been remarked as a noteworthy epoch for the appearance of meteors. The history of this December shower is as follows.

In the year 901, on November 30, "the whole hemisphere was filled with those meteors called falling stars from midnight till morning, to the great surprise of the beholders in Egypt."

930, Nov. 29.—"People saw a large number of little stars which were shooting and falling."

1571, Dec. 8, was the date of another display of shooting stars.

Prof. Newton reduced these dates to the epoch of 1850, finding that they were equivalent to December 13·3, 11·6, and 11·5 respectively. Hence there is little doubt but that the meteor-showers in question were early appearances of the Geminids.

In 1833, about the same date, as many as ten meteors were seen simultaneously, and a similar phenomenon was observed in 1836 on December 11.

1838.—After observing the return of the Andromedids on December 6, 7, and 8 of this year, Herrick and his assistants, at Newhaven (Conn.), U.S.A., recorded 18 meteors on December 11 from $8^h\ 45^m$ to 10^h, 28 on December 12 from 6^h to $13^h\ 30^m$, and 9 on December 15 from 6^h to $9^h\ 15^m$. These figures seemingly indicate a moderate display of Geminids.

1841, Dec. 10, 11.—Colla, at Parma, noted an unusually large number of meteors; in $\frac{1}{2}^h$ on the night of the 11th "23 very brilliant meteors, nearly all of them having luminous trains," were seen in the northern quarter of the heavens, their general direction being from S. to N. An assistant reported that "meteors pursuing the same direction were uncommonly abundant" on the following morning. The same observer recorded many shooting stars on 1846, December 9, 10.

A year later, 1847, December 12, many falling stars were noticed in the constellations Orion, Taurus, Gemini, and Auriga, by E. J. Lowe, Nottingham.

1848, Dec. 11, $5^h\ 38^m$–$6^h\ 50^m$.—10 meteors, many large, with trains.—E. HEIS, Aix-la-Chapelle.

Dec. 14, 15.—Many meteors.—COLLA.

1855, Dec. 12.—Numerous shooting-stars were recorded.—E. J. LOWE, Beeston.

1860, Dec. 12, $8^h\ 20^m$–12^h.—180 meteors, 60 the maximum hourly rate, at $11^h\ 30^m$.—F. MILLER, Montgomery Co., Maryland, U.S.A. (Several students assisted.)

1862, Dec. 10.—In 3^h between 6^h and 12^h, ten or twelve meteors of the first magnitude were seen in all quarters of the sky. "The radiant-point was perfectly marked between Auriga and Gemini." The Geminid radiant-point was first determined in this year, by R. P. Greg, at Prestwich, Manchester, his observation being here given, and by Profs. Twining and Marsh, in the United States.

1863, Dec. 12, $9^h\ 20^m$–$11^h\ 16\frac{1}{2}^m$: 13 meteors. Dec. 13, $8^h\ 11^m$–$10^h\ 44\frac{1}{2}^m$: 4 meteors. The radiant-point was at $105^\circ{\cdot}5\ +30^\circ{\cdot}5$, apparently the first definite position recorded.—Prof. A. S. HERSCHEL, Hawkhurst, Kent.

In 1864, Geminids were again fairly numerous, while in 1866, on December 10, 15 were counted between 11^h and 12^h by T. H. Waller, York. Two nights later, at about 11^h, meteors were seen falling at the rate of one per minute. These were "blue and white, of momentary duration, and the majority without trains."—W. H. WOOD, Birmingham.

The same observer found the horary rate of meteors on 1868, Dec. 11, about 12^h–13^h, to be 20; 53 per cent. were Geminids. Their description tallies with that of two years earlier—"white, with an occasional bluish tint; brief in duration and trainless." On 1870, Dec. 12, during $\frac{3}{4}^h$ of observation between $11^h\ 30^m$ and $12^h\ 30^m$, five meteors were noted. These, again, were "white or blue, and trainless." On the same date 25 Geminids were recorded by Captain (now Lieut.-Colonel) Tupman in the Mediterranean.

1871, Dec. 12, $9^h\ 45^m$–$11^h\ 30^m$.—14 meteors (13 Geminids), mostly small with short paths and moderate velocities.—R. P. GREG, Buntingford.

10^h–$11^h\ 30^m$.—7 Geminids, nearly as bright as first magnitude stars.—R. McCLURE, Glasgow.

$10^h\ 15^m$–12^h.—30 shooting-stars (14 before 11^h), mostly Geminids, about half of which were brighter than the second magnitude, white, and in general not swift.—Prof. A. S. HERSCHEL.

1873, Dec. 10, 11.—23 and 39 meteors respectively. The percentage of Geminids was 59.—J. E. CLARK, Heidelberg.

14 meteors on the former date and 13 on the latter, besides 10 on December 12.—T. H. WALLER.

16 on December 11 and 24 on December 12.—W. H. WOOD.

The numbers of meteors as bright or brighter than the first magnitude given in the accounts of the three observers were 21, 27, and 28, most of these being Geminids.

1876.—Geminids were seen as early as November 21 and December 4.

Dec. 11, 11^{h}–13^{h}.—106 meteors, the majority of which were Geminids with very short tracks, were counted by Perrotin and Jean at Toulouse Observatory.

Dec. 12.—24 Geminids.—W. F. Denning, Bristol.

The Geminids were "quick, short, and white, without trains or streaks except in the case of the larger ones."—H. Corder, Writtle.

1877, Dec. 9, 9^{h}–11^{h}.—Nearly 20 meteors, and on December 10, 7^{h}–11^{h} 30^{m}, 40 out of 65 were Geminids, 25 being seen in the last hour. These were mostly small, and very few left streaks.—H. Corder.

Dec. 9, 11.—30 Geminids.—R. P. Greg.

Dec. 11, 14^{h} 30^{m} to daybreak.—Meteors, probably Geminids, were estimated to be falling at the rate of 50 per hour.—Prof. Lewis Swift, U.S.A.

1879, Dec. 7, 7^{h}–11^{h}: 3 Geminids. Dec. 9, 8^{h}–10^{h}: 2. Dec. 12, 8^{h}–11^{h} 30^{m}: 15. "Very short, quite bright, and very rapid." The brightest were of an emerald-green colour. In all 64 meteors were seen.—E. F. Sawyer, Cambridgeport (Mass.), U.S.A.

Dec. 10, 10^{h} 15^{m}–13^{h} 45^{m}.—69 meteors (36 Geminids).—H. Corder.

Hardly any Geminids were recorded in the four following years.

On 1884, Dec. 11, 10^{h} 25^{m}–11^{h} 25^{m}.—10 Geminids: "very rapid; short paths, quite bright."—E. F. Sawyer.

1885.—Indications of a shifting radiant were observed in this year. Thus, from December 1 to 4, Geminids radiated from 105° +34°, and from December 9 to 10 from 107° +33°.—W. F. Denning.

1887, Dec. 11, 16^{h} 30^{m}–17^{h} 30^{m}.—Large numbers of meteors were seen at Meximieux (Ain) and Beaulieu (Alpes-Maritimes).

Dec. 12, 13.—Numerous bright meteors were observed at Dresden.

1892, Dec. 9: 40 Geminids. Dec. 12, 6^{h}–8^{h}, 10^{h}–13^{h} 15^{m}: 66 meteors (38 true Geminids, 12 from another centre in Gemini).—H. Corder, Bridgwater.

9^{h} 45^{m}–11^{h} 30^{m}.—20 meteors (6 true Geminids).—W. F. Denning.

10^{h} 45^{m}–14^{h} 38^{m}.—24 meteors, and 5 earlier in the evening (6 Geminids).—Prof. A. S. Herschel, Slough.

1894, Dec. 12, 19^{h} 30^{m}.—In about 10^{m} at this time not less than 20 meteors were counted in the N.E. sky by an observer at Montreux, Switzerland.

1895, Dec. 10, till midnight: 31 meteors. Dec. 12, 13^{h} 45^{m}–18^{h}:

88. From December 10 to 15 the number of Geminids seen was 78.—H. Corder.

Dec. 11.—More than 50 meteors.—Rev. T. H. Foulkes, Devonport.

Dec. 12, 40^m near midnight.—20 meteors (horary rate of Geminids 25).—Prof. A. S. Herschel.

$11^h\,10^m$–$12^h\,26^m$.—24 Geminids.—E. R. Blakeley, Dewsbury.

1896, Dec. 9, $8^h\,30^m$–13^h (at intervals): 39 meteors (22 Geminids). Dec. 11, 10^h–15^h: 111 (89). Dec. 12, 14^h, 15^h–$16^h\,15^m$: 30 (21); 9 were of the 1st magnitude, and only three left streaks. At midnight on December 11 the horary rates for all meteors and Geminids were 30 and 20 respectively.—H. Corder.

Dec. 11.—About 20 Geminids.—D. Booth, Leeds.

About midnight 30 per hour, all meteors (20 Geminids).—E. R. Blakeley.

$11^h\,55^m$–$12^h\,25^m$: 9 meteors. Dec. 12, 10^h–12^h: 10 or 12 Geminids.—Prof. A. S. Herschel.

1898.—A few Geminids were recorded on December 9.

Dec. 12, in $2^h\,5^m$ between 7^h and $13^h\,22^m$, 19 meteors out of 25 were Geminids.—T. W. Backhouse, Sunderland.

$7^h\,25^m$–11^h.—52 meteors.—R. Service, Dumfries.

$9^h\,5^m$–$10^h\,5^m$.—36 meteors (35 Geminids).—V. Nielsen, Hartlepool.

$9^h\,30^m$–$12^h\,30^m$.—41 meteors.—A. King, Leicester.

$12^h\,30^m$–$14^h\,45^m$.—5 true Geminids, and 12 from other radiants in Gemini.—W. E. Besley, Westminster.

1899, Dec. 8 and 9.—A considerable number of Geminids.—T. H. Astbury, Wallingford.

Dec. 10, $12^h\,10^m$–$13^h\,30^m$.—17 meteors (15 Geminids): short paths; several appeared almost stationary.—I. F. H. C. Gregg, Malvern.

Prof. Kirkwood has tabulated the early appearances of the Geminids in the same manner as those of the Lyrids, and deduces a period of about $29\frac{1}{8}$ years, thus:—

A.D. 901 to 930 = 1 period of 29·000 years.
930 to 1571 = 22 periods of 29·136 years.
1571 to 1833 = 9 periods of 29·111 years.
1833 to 1862 = 1 period of 29·000 years.

This period is to a large extent borne out by the strength of the display of 1892, though this was apparently exceeded by those of 1895 and 1896. It is a curious fact that the return of 1862 was also followed in four years by another striking display of Geminids.

From the accounts of the various observers detailed above it will have been gathered that the Geminids are usually short and quick, streak-leaving only as regards the brighter members of the shower, and that not invariably, and of a pale tint, the most brilliant being sometimes of a vivid green. The average speed of six meteors of this class of which the real paths have been calculated is 40 miles per second.

The mean centre of 61 radiants of Geminids collected by Mr. Denning in his 'General Catalogue' is 106°·0 + 32°·4 (Radiant No. lxxxviii.). This is very near the earliest position recorded—that by Prof. A. S. Herschel in 1863. Meteors have been seen issuing from the vicinity of this radiant on many dates between September and March.

The point as to which observations are needed is that as regards the suggested shifting character of the radiant. For this purpose watches should be kept for the shower on several nights preceding and following the date of maximum.

75 The Chase, Clapham Common. W. E. Besley.

Changes in the Stellar Heavens.

Astronomers would like to know what the appearance of the heavens was about the dawn of human history, say 6000 years ago, and whether any of the stars then visible have since disappeared, or have increased or diminished in brightness. But no such knowledge is available, either in the form of maps or catalogues, and we must be content with the catalogues and descriptions which have been made in more modern times.

The first known catalogue of stars was made by Hipparchus about 134 B.C. The next catalogue was constructed by Ptolemy, who observed at Alexandria between the years A.D. 127 and A.D. 151, and is contained in the 7th and 8th books of the famous 'Almagest.' It seems doubtful, however, that Ptolemy's catalogue represents genuine observations made by Ptolemy himself, or is merely the catalogue of Hipparchus slightly modified. However this may be, we have a catalogue or description of the heavens made in the tenth century by the Persian astronomer Al-Sufi, which is undoubtedly the result of original observations made by Al-Sufi himself, and it is the oldest catalogue on which we can place any reliance. This most interesting and valuable work is preserved in two Arabic manuscripts, one in the Royal Library of Copenhagen and the other in the Imperial Library of St. Petersburg*. A French translation of these manuscripts was published in 1874 by the Danish astronomer Schjellerup, and some account of a comparison I have made of this work with modern observations may prove of interest.

Not very much is known of the details of Sufi's life. His full name was Abd-al-rahman Bin Umar Bin Muhammed Bin Sahl

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* There are also three copies of Al-Sufi's work in the Imperial Library of Paris, but these are inaccurate. There is also one in the British Museum and another in the Indian Office Library; but these are imperfect, considerable portions of the original work being missing.

Abu'l-husaïn al-Sûfi al-Razi. The name Sûfi indicates that he belonged to the sect of Sûfis, and the name Razi that he lived in the town of Raï in Persia, to the east of Téhéran. He was born on Dec. 7, A.D. 903, and died on May 25, 986, so that, like many other astronomers, he lived to a good old age. According to ancient authorities, Al-Sufi was a very learned man, who lived at the courts of Shiraz and Bagdad under Adhad-al-Davlat of the dynasty of the Buïdes, who was then the ruler of Persia. He was held in high estimation and great favour by this prince, who said that "Abd-al-rahman al-Sufi taught me to know the names and positions of the fixed stars, Scharif Ibu al-Aalam the use of astronomical tables, and Abu Ali al-Farisi instructed me in the principles of grammar." Prince Adhad-al-Davlat died on May 26, 983 A.D.

Al-Sufi's description of the fixed stars was, it is true, founded on Ptolemy's catalogue, but it is not a mere translation of the work of his predecessor. It is, on the contrary, a careful and independent survey of the heavens made from his own personal observations, each of Ptolemy's stars having been carefully examined as to its position and magnitude; and Ptolemy's mistakes corrected. Al-Sufi's work also contains the magnitudes given by Ptolemy (or Hipparchus), which makes it still more valuable, as Ptolemy's magnitudes given in all the editions of the 'Almagest' now extant are quite unreliable.

In the preface to his work, Al-Sufi says *:—

"Ce livre renfermera la description des quarante-huit constellations, traitera des étoiles de chaque figure, de leur nombre, de leurs positions par rapport à la figure elle-même et au zodiaque en leurs longitudes et leurs latitudes, enfin du nombres de toutes les étoiles qui ont été observées, soit dans les constellations, soit hors des constellations."

Al-Sufi's description of the position of each star he observed is generally so clear that there is no difficulty in identifying the star referred to. With reference to these positions, compared with the positions given by Ptolemy, he says:—"Entre l'époque pour laqu'elle nous avons fixé les positions des étoiles dans ce livre, c'est-à-dire l'an 1276 d'Alexandre † (*dzû'l-karnaïn*) et l'observation de Ménélaüs, il s'est écoulé huit cent soixante-six ans. Le mouvement des étoiles dans cet intervalle, en le supposant d'un degré en soixante-six ans, a dû être de 13° 7′ environ. Si nous en retranchons 25′, l'addition faite par Ptolémée à chaque étoile, il nous restera ce qu'il faut ajouter aux lieux assignés par Ptolémée dans son livre, c'est-à-dire 12° 42′. Le lieu d'*al-simâk* sera donc, pour le commencement de l'an 1276 d'Alexandre, dans 9° 22′ de la Balance, et la plus boréale des trois du front du Scorpion se trouvera dans 19° 2′ du Scorpion: et telle sera l'addition à faire à toutes les autres étoiles.

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* In all the extracts from Al-Sufi's work in the present paper, I quote from Schjellerup's French translation of the Persian manuscripts.

† This date corresponds to the beginning of the year 964 A.D.

"Quant aux latitudes, elles seront les mêmes que celles de Ptolémée, parce que les étoiles tournant autour des pôles du zodiaque, les latitudes ne changent pas.

"Quant aux grandeurs des étoiles nous le marquerons *telles que nous les avons vues de nos propres yeux.*" *

As an example of the care and accuracy of Al-Sufi's descriptions of the stars, I quote the following with reference to the stars in the north of Serpens, which lies south of Corona Borealis. If the reader will compare Al-Sufi's description with a star atlas he will see how careful and accurate Al-Sufi was:—

"Entre la 4e [β Serpentis] qui se trouve au commencement du cou et cette 7e étoile [δ Serpentis], il y a une [χ Serpentis] de la cinquième grandeur dont Ptolémée n'a pas parlé. Elle est éloignée, à vue d'œil, environ d'une coudée de la 4e vers le sud. Puis le Serpent s'étend vers le sud, en s'inclinant un peu vers l'orient, jusqu'aux deux étoiles contiguës, dont la plus boréale est de la quatrième grandeur, savoir la 8e [λ Serpentis], la 9e [α], la plus méridionale, est de la troisième grandeur. La 10e [ε] reste en arrière de ces deux étoiles, s'inclinant, à vue d'œil, à une coudée de la 9e vers le sud. Elle est des moindres de la troisième grandeur, tandis que Ptolémée la dit absolument de troisème. La plus brillante de ces trois étoiles est la 9e [as it is to-day], et c'est elle que l'on marque sur l'astrolabe et que l'on nomme *nuk al-haija*, le cou du Serpent. Les 7e, 9e et 10e forment une ligne d'une légère courbure, de manière que la ligne droit qui joint la 7e avec la 10e, passera entre les deux contiguës, savoir les 8e et 9e, laissant vers le sud la brillant 9e étoile."

The above is a good specimen of Al-Sufi's descriptions of the stars, and a very clear and accurate account of the positions of the stars referred to as they appear at present. The magnitudes assigned to these stars by Al-Sufi are also in fairly good agreement with modern estimates and measures.

With reference to the "coudée," the measure of apparent distance generally used by Al-Sufi, he says, in describing the constellation Auriga, "Une coudée entre deux étoiles culminantes mesurée en degrés sur l'une des grands cercles qui se trouvent sur les globes, est à peu près 2° 20′." He sometimes speaks of an "empan" as the distance between two stars. This is one-third of a "coudée," or about 47′ of arc.

As I have said, most of Al-Sufi's stars can be easily identified on modern star maps, or in the sky; but this identification is in a few cases difficult and puzzling, and in the following pages I will consider the most remarkable of these difficulties together with other points of interest. I will also consider discrepancies between the magnitudes of certain stars as given by Al-Sufi and those determined by modern estimates and photometric measures. I will take the constellations in the order given by Al-Sufi.

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* The italics are mine.

Ursa Major, or the Great Bear.—With reference to the faint star Alcor, near ζ, which was called *al-suha* by the Arabians, Al-Sufi quotes the Arabian proverb "Je lui fais voir *al-suha* et il me montre la lune," which seems to show that it was a conspicuous object to the Arabian astronomers. Sufi says that the Arabs called the well-known pairs of stars below "the Plough" (viz.: νξ, λμ, and ικ) by the name of "Sauts des Gazelles," νξ being the first Saut, λμ the second Saut, ικ the third Saut. With reference to eight stars outside the old figure (the externs, as Al-Sufi calls them), there is considerable difficulty in identifying two of them, viz. the sixth and seventh. The first of these eight stars is clearly α (Flamsteed 12) Canes Venatici, and the second β of the same constellation. The latter star has probably somewhat increased in brightness since Al-Sufi's time, as he rates it 5th magnitude, whereas it is now nearer 4. The 3rd and 4th stars are Flamsteed 40 and 38 Lyncis respectively, and the 5th seems to be Fl. 10 of Leo Minor. The 6th and 7th are not identified by Schjellerup, but from the positions given by Al-Sufi, the 6th may perhaps be 10 of Ursa Major and the 7th Fl. 35 of the Lynx. 10 is now about the 4th magnitude, as stated by Al-Sufi. The 5th, 6th, 7th, and 8th stars were not mentioned by Ptolemy.

Draco.—In the case of the stars numbered 10 and 11 in this constellation by Al-Sufi, there is a remarkable discrepancy between his estimates of their magnitude and the results of modern estimates and photometric measures. Al-Sufi says: "L'une des ces étoiles, la 10^{e}, est la plus méridionale du cote antérieur du quadrilateral et des petites de la troisième grandeur. Ptolémée la dit de la quatrième. La 11^{e}, la plus boréale du même cote, est de la quatrième grandeur." From this it is clear that 10 is π Draconis and 11 δ Draconis. Now π was estimated 5th magnitude by Argelander and Heis and was measured 4·55 at Harvard, whereas δ was rated 3rd magnitude by Argelander and Heis and was measured 3·21 at Harvard. It seems probable therefore that one or both of these stars may have changed in brightness since Al-Sufi's time, unless, indeed, Al-Sufi has, by mistake, interchanged the magnitudes of δ and π, which seems improbable.

With reference to the stars φ and χ Draconis, Al-Sufi says that φ "est la plus boréale* des deux et la plus brillante." But modern estimates and measures make φ decidedly the fainter of the two. φ is a suspected variable. The star ω Draconis has probably brightened since Al-Sufi's time, for Ptolemy and Al-Sufi made it 6th magnitude, whereas Argelander and Heis rated it 5, and it was measured 4·86 with the photometer at Harvard.

Boötes.—Both Ptolemy and Al-Sufi rated ε Boötis 3rd magnitude and σ 4 m. At present ε is about 2 magnitudes brighter than σ.

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* That is nearer to the pole of the *ecliptic*, not to the pole of the equator. Recent spectroscopic observations by Prof. Campbell show that χ Draconis is a close binary star. The period is about 282 days.

Corona Borealis.—Both Ptolemy and Al-Sufi make ϵ and ι 4th magnitude; but at present ϵ is about one magnitude brighter than ι, which has probably faded since Al-Sufi's time.

Hercules.—Ptolemy and Al-Sufi made μ and ν the same magnitude, but μ is now at least a magnitude brighter than ν. Al-Sufi made ω Herculis 4th magnitude and χ Herculis 5th magnitude, reversing Ptolemy's estimate, but now the relative brightness is again reversed.

Cygnus.—The star Flamsteed 47 of this constellation would seem to have brightened since Al-Sufi's time. He estimated it 6th magnitude, but it was rated 5–6 by Argelander and Heis, and was measured 4·77 at Harvard, or nearly equal to Flamsteed 39, which Al-Sufi rated 4–5. There can be no doubt as to the identity of the star referred to by Al-Sufi, for he says: "au-devant de la 10^e [ϵ Cygni] du coude gauche se trouvent quatre étoiles de même forme qu'*al-avaïdz*, qui consiste des quatres étoiles dans la tête du Dragon. Les deux précédentes près l'une de l'autre, sont des petites de la quatrième grandeur, la méridionale (Fl. 41) étant un peu plus brillante [as it is at present], et les deux suivantes ont leur distance mutuelle plus large. La méridionale de ces [Fl. 52] deux suivantes est la cinquième grandeur, et la boréale [Fl. 47] de la sixième." It is curious that Al-Sufi does not mention ρ Cygni, which lies not far from ξ [Al-Sufi's 14th star]. ρ is now about the 4th magnitude and slightly brighter than σ and υ Cygni, which Al-Sufi mentions, and rates as 4–5 magnitude.

Cassiopeia.—Schjellerup identifies Al-Sufi's 8th star of this constellation with μ. I prefer θ, however, as Al-Sufi's description applies equally well to it, and as it is brighter than μ it was probably the star observed by Al-Sufi. The two stars lie close together, and it seems altogether improbable that Al-Sufi should have recorded the fainter star and omitted the brighter. Further, the magnitude given by Al-Sufi, 4·5, is quite correct for θ. Possibly, however, Al-Sufi may have seen the two stars as one. Schjellerup makes the 9th star θ, but from Al-Sufi's description it is clearly ϕ Cassiopeiæ, as pointed out by Peirce some years ago. Al-Sufi's 13th star, ρ Cassiopeiæ, seems to have increased in brightness since his time. He says it "est une petite étoile de la sixième grandeur." But it was rated 5th magnitude by Argelander, 5–4 by Heis, and was measured 4·56 at Harvard. J. E. Gore.

[To be continued.]

*James Edward Keeler**.

The announcement of the death of Prof. J. E. Keeler, Director of the Lick Observatory, which occurred on August 12th last, must come as a surprise and a shock to all. He was barely 43 years of age. It was known to his friends and colleagues that he had been suffering from a severe cold, but there had been no intimation that a serious outcome was even remotely to be feared.

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* From *Nature*, Sept. 20. Abridged from Obituary Notice by Prof. G. E. Hale.

James Edward Keeler was born at La Salle, Illinois, on September 8, 1857. His qualifications for scientific work clearly showed themselves at the John Hopkins University, where he took an undergraduate course, and served as assistant to Prof. Hastings, with whom he observed the total solar eclipse of 1878 in Colorado.

Shortly after this he was appointed assistant at the Allegheny Observatory, where he had an important part in the long series of bolometric investigations carried on by Prof. Langley, then director of the Observatory. In July 1881 he was a member of Prof. Langley's well-known expedition to Mount Whitney, in Southern California, where an extensive region in the extreme infra-red of the solar spectrum was discovered with the bolometer. Later he studied for two years in Berlin and Heidelberg under Helmholtz and Quincke, and returned to the Allegheny Observatory, remaining here until appointed a member of the staff of the Lick Observatory. His work on Mount Hamilton commenced in 1885, and for some time he was the only astronomer at the Observatory, which was still in process of construction. In May 1891 he was elected professor of astrophysics in the Western University of Pennsylvania and director of the Allegheny Observatory.

Keeler's work at the Lick Observatory was continued in a most effective manner with the modest instrumental resources at Allegheny. With a full understanding of the art of making the most of his means, he took up photography for the first time, made himself thoroughly familiar with photographic processes, and then, with the aid of a spectrograph whose general design has been followed in the construction of the great modern spectographs at Mt. Hamilton, Potsdam, Poulkova, and Williams Bay, he obtained the photographs of the spectra of red stars which excited so much interest at the dedication of the Yerkes Observatory. He also made an admirable series of drawings of Mars, which was published in the *Memoirs* of the Royal Astronomical Society.

In the spring of 1898 Keeler had practically decided to accept a position on the staff of the Yerkes Observatory, and would have done so had he not just then been appointed director of the Lick Observatory. His recent work on Mt. Hamilton has not been confined to the direction of the affairs of a great observatory. The remarkable success of his experiments with the Crossley reflector, of which a full account is fortunately preserved in the June number of the *Astrophysical Journal*, has impressed every one who has seen the wonderful photographs of nebulæ and star-clusters made with this instrument.

Of Keeler's other contributions to science two in particular deserve present mention; his determination with the Lick telescope of the motion in the line of sight of the planetary nebulæ, and his demonstration of the meteoric constitution of Saturn's rings. The memoir which describes the first of these investigations already ranks as a classic of astrophysical literature; while the spectroscopic demonstration of the meteoric constitution of Saturn's ring is perhaps the most striking of the many effective applications which have been made of Doppler's fruitful principle.

Much more might be said of Keeler's work, but this should suffice to indicate its lasting value. It is a satisfaction to add that its merit has been widely appreciated, as has recently been evidenced by the award of the Draper and Rumford Medals. He was President of the Astronomical Society of the Pacific and a councillor of the Astronomical and Astrophysical Society of America. He was elected an Associate of the Royal Astronomical Society in 1898 and a member of the National Academy of Sciences at its last meeting. His kindly and genial manner, combined with unusual tact and rare judgment, drew to him many friends, who will long mourn his loss.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Total Eclipse of 1851.

Gentlemen,—

The owner of the now famous Note-Book asks in your June number (vol. xxiii. p. 262), "How often, if ever, has the track of totality included an observatory of any note?" Mr. Crommelin, in the following number (p. 288), reminds us that the total eclipse of 1715 was observed by Flamsteed at the Royal Observatory, Greenwich. Perhaps I may be allowed to call attention to the fact that the totality of that of July 28, 1851, passed over Königsberg, which had been made so illustrious by the labours of Bessel, but who had died about five years before. The eclipse, however, was observed by Dr. Busch, then the Director, and by his request a daguerrotype of it was taken by Berkowski with the heliometer, which is the earliest instance of the application of photography to solar eclipses. It was taken after an exposure of 84 seconds, and an engraving from it is given in the 26th volume of the Königsberg observations, which is reproduced in Ranyard's well-known eclipse-volume (vol. xli.) of the 'Memoirs of the Royal Astronomical Society.' It is there attributed to Dr. Busch, as it also is in Mrs. Todd's valuable work on 'Total Eclipses of the Sun' (p. 120). Busch, indeed, discusses it in the above volume of Königsberg observations, but does not omit to tell us that actual artist was Berkowski, whom he speaks of as one of [illegible] skilful daguerrotypists in the town. In comparing the result with photographs taken during later eclipses, it is of course to be remembered that on that occasion a maximum of solar spots had passed about two years before.

Yours faithfully,
W. T. LYNN.

Blackheath, 1900, Aug. 8.

The Lost Pleiad.

GENTLEMEN,—

It is well known that the attribution of seven stars to the Pleiades was very widely diffused in ancient times, and also that one, thence called the lost Pleiad, had become invisible, for which a mythological reason was invented. Aratus, however, seems to have thought that the idea was simply a blunder, and that it was impossible one could have been really lost. The line in Ovid's 'Fasti' (iv. 170),

> Quæ septem dici, sex tamen esse solent,

is quoted by Airy in the paper in the *Monthly Notices* (vol. xxiii. p. 175), in which he says that a member of his family marked the positions of twelve seen with the naked eye, which were afterwards identified by himself. Hyginus, who was contemporary with Ovid, writes in his 'Astronomicon' (ii. 21):—

> Hæ numero septem dicuntur, sed nemo amplius sex potest videre.

Nevertheless, so firm was the belief that there were or had been seven, that "the seven stars" was one of the designations of the cluster. That it is referred to in Job ix. 9, xxxviii. 31, and Amos v. 8, under the Hebrew word *Kimah*, there can be little doubt. The Vulgate, however, takes it to be Arcturus, but Luther (influenced probably partly by the Septuagint) substitutes "die Glucke," the clucking hen, the hen and chickens being a well-known designation of the Pleiades. Coverdale evidently follows this, for he translates "the seven starres," and affixes a marginal note to Job ix. 9, "Some call these seven starres the cluck-henne with hir chekens." The Authorized Version substitutes "the Pleiades" in both the above places in Job, but retains "the seven stars" in the passage in Amos, undoubtedly meaning by the expression the Pleiades, by which the Revisers translate the Hebrew word throughout.

The tradition that there were or had been seven stars must have been founded on some persons having seen or thought they saw that number in the group. But it is surely very remarkable that there is no record of more than six having been seen until Mästlin, the preceptor of Kepler, who in 1579 registered the positions of eleven (see Winnecke's paper in *Monthly Notices*, vol. xxxix. p. 146, where these are given from the 'Historia Cœlestis' of Albert Curtz or Curtius, which appeared in 1666), and he is stated, though somewhat doubtfully ("nisi fallor") by

Kepler (Frisch's edition of his works, vol. ii. p. 500) to have seen no fewer than fourteen.

Miss Clerke, in the chapter (xv.) on the Pleiades, in her 'System of the Stars' (p. 220), points out the changes of brightness that must have taken place in several of the stars in the cluster. Ptolemy catalogues only four, and if Alcyone (now so much the brightest) is one of these, it was then of much feebler light than it is now. She therefore thinks there is much to be said for Prof. Pickering's suggestion (*Ast. Nach.* No. 2934, vol. cxxiii. col. 95) that the star now called Pleione is really the lost Pleiad. This is founded on a peculiarity in its spectrum, similar to that of P or 34 Cygni *, which underwent remarkable changes of brightness in the seventeenth century, but has remained of the same magnitude (about 5½) since. "A similar change in Pleione," remarks Prof. Pickering, "would render seven stars visible in the Pleiades to ordinary eyes." That seven really were so in former times can scarcely admit of doubt. Yours faithfully,

Blackheath, 1900, Sept. 3. W. T. Lynn.

Prevalence of Fireballs on Sundays.

Gentlemen,—

The talented and genial author of the "Note-Book" refers (*Observatory*, September) to an instance in which it would appear, from the very suggestive names, that a meteor had selected the places over which it passed! This allusion leads me to remark upon another feature in regard to apparitions of meteors, viz., the disposition of fireballs to appear on Sunday evenings! My attention was drawn to the circumstance many years ago, and experiences of the last few months seem to have amply supported the idea, large meteors having appeared as follows:—

1900.	h	m	Estimated brilliancy.
Sunday, June 10....	9	10	1½ × ♀
,, 24....	10	57½	> ♀
,, July 15....	10	13	2 × ♀
,, Aug. 12....	9	27	= ♀
,, 12....	10	21	> ♀
,, 19....	10	36	3 × ♀
,, Sept. 2....	6	54	> ☽
,, 9....	7	45	Brilliant
,, 9....	9	8	= ♀
,, 16....	8	44	2 × ♀

During recent years a considerable number of these bodies have been visible on sabbath evenings, and the following are a few brilliant specimens:—

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* Perhaps it may not be out of place to refer here to two letters of mine on this star in the *Observatory*, vol. ix. p. 314, and vol. xiii. p. 244.

Year.	Date.	h	m	Year.	Date.	h	m
1893....	Oct. 1	9	40	1898....	Feb. 20	8	54
1894....	April 22	7	36	1898....	Aug. 21	9	16
1894....	Aug. 26	10	20	1898....	Sept. 18	8	54
1895....	July 7	10	49	1899....	Mar. 12	9	0
1896....	April 12	8	6	1899....	Aug. 27	10	13
1897....	Dec. 12	8	6	1899....	Oct. 8	6	3

But it is difficult to find any effective cause for an undue prevalence of fireballs on any particular day of the week. There may be something in the fact that in towns there is less artificial light on Sunday evenings, and people might more readily notice meteoric appearances than on other nights. Moreover, many persons stroll about in the suburbs, and would be likely to witness any conspicuous appearance in the sky.

I have amused myself by consulting a large number of fireball records for the years previous to 1890, and found the days of the week on which they appeared. There seems to be little corroboration of the view that Sunday offers a favourable night. No doubt the idea is a mere impression made upon the mind by a few striking instances, and could not be substantiated by an exhaustive search among past observations.

Another opinion which I and some other people have encouraged is that Sundays are generally clearer and afford more sunshine than any other day of the week. This is doubtless a mistaken conclusion, induced by the remembrance of many fine Sundays when leisure has enabled us to fully appreciate them. I have never consulted meteorological records in reference to the point, but it seems to me that it would be only time wasted were any serious attempt made to ascertain whether fireballs or fine weather showed a predilection for Sundays. But some of our impressions, though probably erroneous, are apt to linger in the mind, and we find it difficult to put them entirely on one side.

Bishopston, Bristol,
Sunday, Sept. 23.

Yours faithfully,
W. F. Denning.

Position of the Red Spot on Jupiter relative to the Hollow of the S. Equatorial Belt.

Gentlemen,—

The question of the position of the great red spot on Jupiter with respect to the deep hollow or bay in the S. equatorial Belt opposite to it in 1899 has been discussed by Mr. W. F. Denning in *The Observatory* for the current year, pp. 216, 286, and by the writer on p. 255. This question is one of real importance in connection with investigations into the varying rate of rotation or motion of the spot, otherwise I should not have returned to the subject. For nearly 20 years observers have been in the habit of observing the spot exclusively, but during the past two or three years some observers have preferred to observe the transits of the hollow instead of the spot itself, on account of the faintness of the latter. It is obvious, therefore, that the question of the position of the spot relative to the hollow will enter into every attempt to connect the observations of the former with those of the latter.

It will, perhaps, be remembered that I suggested, as an explanation of the difference between the observations of Mr. Denning and myself, that possibly the full length of the spot may not have been seen by Mr. Denning on account of the faintness and indefiniteness of its extreme preceding end. At the time I could only venture to suggest this as a probable explanation of the difference, but I am now able to refer to a paper by Prof. G. W. Hough, in the May number of the *Monthly Notices*, which I think may be taken as evidence in the matter.

Prof. Hough has discussed his measures of the length of the red spot made with an 18½-inch refractor and comes to the conclusion (p. 548) that probably the spot "has not materially changed in size during the past 20 years." Mr. Denning, in criticizing my observations, appears to think that I observed the spot when faint of too great a length, whereas I measured it to be no longer than when it was well-defined, so the inference is that Mr. Denning himself has not seen the whole length of the spot during its recent period of faintness, and an examination of the drawing of Jupiter by him in the September number of *Knowledge* will show that a *very slight* extension of the faint and somewhat indefinite preceding end would cause the spot to transit slightly before the hollow.

I think it may therefore now be regarded as established that in 1899 the centre of figure of the red spot did really slightly precede the centre, or middle point, of the hollow, though the difference was probably only very slight indeed. It is at any rate quite certain that the spot did not follow the hollow.

Yours faithfully,
A. Stanley Williams.

Hove, 1900, Sept. 17.

OBSERVATORIES.

These brief reports of the work done at various Continental Observatories during the year 1899 have been abstracted from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' vol. 35.

Bamburg. *Ernst Hartwig.*—The Schröder refractor has been used by Dr. Clemens for astronomical photography, with which he succeeded in detecting a variable in the Pleiades (B.D. +24°·531). Much time was given to observations of variables. Diameters of Sun and Planets, position of Crater Mösting A, and observations of Comets Swift and Giacobini. Meteorology. Time service.

BERLIN. *W. Foerster.*—Prof. Batterman observed with the large meridian circle. Prof. Knorre, with a double-image micrometer attached to the 6-inch refractor, continued his measures of double stars. Herr Foerster made parallax observations of bright stars with small proper motions, on the supposition that the apparent fixity was due to a more or less coincidence of motion with our system. The annual P.M. of α Cygni is 0″·004, its motion in the line of sight 8 km. per second; hence if we were looking from a direction at right angles to our present direction, this P.M. would be some 40 times larger, and the star would have an annual parallax of 0″·10. The work will be continued.

BONN. *F. Küstner.*—The observatory has been enriched by the installation of a visual and a photographic telescope on the same mounting. The lenses are by Steinheil, and were mounted by Repsold.

The visual lens is 360 mm., with focal length 5440 mm.

The photographic lens is 300 mm., with focal length 5130 mm.

With the transit-circle over 3000 star-positions have been obtained. Prof. Deichmüller spent large part of his time on star magnitudes for the Bonn A. G. Catalogue. Prof. Mönnichmeyer looked after the meteorological work.

BRESLAU. *J. Franz.*—Co-ordinates of 150 Lunar Craters from Lick photographs. The 8-inch Engelmann refractor has been used for measurement of double stars.

DUSSELDORF. *R. Luther.*—94 observations of 34 small planets, making, since 1848, a total number of 2245 observations of 222 planets by three observers only.

GÖTTINGEN. *W. Schur.*—With heliometer Prof. Schur obtained diameters of Sun on 22 days, and diameters of the large planets. Double stars 70 Ophiuchi and 61 Cygni. Parallax measures of 61 Cygni and Polaris. Prof. Ambronn used the great heliometer for similar observations and for comets.

HAMBURG. *R. Schorr.*—Comets and small planets. Organization of observations of November meteors in a number of districts, including arrangements for photographing the shower. At the observatory itself 149 meteors were observed November 15–16. Time service. Chronometer work.

HEIDELBERG (Grand Ducal). *W. Valentiner.*—In the astronomical branch the transit was used for observations of refraction stars, Sun (33 days), comet Swift (11 days). The Steinheil 5-inch and 6-inch refractors for eclipses, occultations, and comets.

HEIDELBERG (Grand Ducal). *Max Wolf.*—In the astrophysical department the 16-inch Bruce telescope was mounted on plans designed by Grubb, and a large number of photographs of different parts of the sky obtained with the aid of the 6-inch Voigtländer portrait-lens, which has been reground and now gives good images. Observations and photographs were also obtained of the Solar Eclipse, 1899, June 7. Great attention has been paid to the Zodiacal Light. The observations of Perseids and Leonids are published in the *Ast. Nach.* Comet Swift was photographed on 6 nights; Tuttle on 1 night; Tempel II. on 2 nights; Holmes 1899 II. on 3 nights; Giacobini 1899 *e* on 6 nights. Drawings were made of Mars, Jupiter, Saturn, and Venus.

JENA (University). *Otto Knopf.*—A large number of observations of small planets were made with the 6¾-inch, including 92 morning and evening observations of Iris for Sun's parallax. Time service. Meteorology.

JENA (Winkler). *W. Winkler.*—The 4-inch triple aprochromatic lens from Zeiss was tested on double stars and found satisfactory. Occultations; measures of double stars. Sun-spots were observed on 230 days.

KALOCSA. *T. Fenyi, S.J.*—Sun's limb examined completely on 160 days and partially on 31 days; on two occasions only, in March, was the limb without a prominence of at least 30″ altitude. Thirteen were noted 100″ alt., and one of 154″. The Sun's image was projected and examined on 220 days, and of these 69 were free from spots.

KASAN. *D. Dubiago.*—The building of the new observatory was commenced 1899, May, and rapid progress made. Prospects of instruments being installed and in work during 1900.

KIEL. *H. Kreutz.*—Publication of the 'Astronomische Nachrichten,' 'Centralstelle für astronomische Telegramme.' It is hoped soon to publish the results of a comparison of the Helsingfors Catalogue with older catalogues—a work on which Herr Möller has been engaged. Orbits of small planets.

KÖNIGSBERG. *H. Struve.*—Latitude variation. Dr. Cohn has employed the 13-inch, measuring 200 interesting double stars; observations of small planets, especially Eros: comets Tuttle, Swift, Tempel, and Giacobini; drawings of Mars. Dr. Cohn also used the 6-inch heliometer for parallax work.

LEIPZIG. *H. Bruns.*—Parallax of σ Draconis. Diameters and drawings to scale of Venus. Lunar surface measurements. Completion of Zone Catalogue, which is to be published in 1900. Time Service. Meteorology.

MILAN. *G. V. Schiaparelli.*—18-inch used for double-star work and observations of surface-markings of Mars. Dr. Celoria used the 8-inch for double stars. Geodetic work. Meteorology.

KODAIKANAL.—Mr. Michie Smith's Report on the Kodaikanal and Madras Observatories, over both of which he has now control, for the year 1899–1900 contains little of astronomical work done, as might be expected, as the Director has been but a short time in his new home, and has been hard at work settling down. The following extracts from the Report are interesting:—

"The scheme for the re-organization of the Indian Observatories having come into operation from 1899, April 1, the Madras Observatory was from that date transferred from the Government of Madras to the Government of India, and the former Government Astronomer became Director of the Kodaikanal and Madras Observatories. Thus ended the connection between the Local Government and the Observatory, which had lasted for 107 years, a period during which much useful work had been done, sometimes, indeed, in face of official discouragement, but usually with the sympathy and support of the Local Government. It remains to be seen whether the evident advantages of the direct connection with the Imperial Government will altogether make up for the drawbacks arising from the greater distance of the governing power."

On March 31st, 1899, the former staff of the Madras Observatory was reorganized. The chief Assistant retired and some received appointments at the new observatory at Kodaikanal. There appeared to be no difficulty in filling the posts of the four Assistants, which are occupied by Indian subjects, three of them being graduates, but it was not so easy to fill the subordinate posts. Mr. Michie Smith says:—

"The mechanic first appointed, who had received a special training at the School of Arts, Madras, took fright at the climate and remained only two days. The man next appointed came part of the way and then turned back. The man finally obtained has not the same qualifications as either of the men previously appointed. Of the peons first appointed, two, who were pensioned Sepoys, left after three days, on the ground that the village was so far away that they could get no one to drink with. Natives who come up here from the plains at first find the climate very trying, but if they can be persuaded to stay for a few months they find that it is not nearly so bad as they at first thought. The appointment of a writer has not yet been made, as there is no house for him to live in."

The completion of the building has proceeded slowly*, the Director, it appears, owing to the lack of skilled workmen, rivetted the domes with his own hands. He has set up clocks and some other instruments, including a seismograph, which appears to be a necessary instrument in the locality, as on 1900, Feb. 8, an earthquake occurred sufficient to crack the arches in the Director's house at Kodaikanal. Beyond some sextant observations for time determination, no astronomical work has been done; but for this we must wait for subsequent Reports.

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* See *Observatory*, 1899, December, p. 453.

Bidston Observatory, Liverpool.—The Report from this Observatory for the year 1899 is essentially of the same nature as that for the preceding year. The transit instrument has been used for determination of time, which with the meteorological registration and chronometer testing forms Mr. Plummer's staple work, and besides this the Right Ascensions of the circumpolar stars in Dr. Auwers' list in *Ast. Nach.* No. 3440 have been determined, the details of which are given in the Report. The equatorial has been used to make observations of Swift's comet and of the diameters of the larger planets, which will be communicated to the Royal Astronomical Society.

PUBLICATIONS.

The Path of the Sun[*].—This title is rather misleading, for at first glance it gives no hint that the book is other than an orthodox work on astronomy; but a second look at the cover, which is adorned by a diagram contrasting "Supposed Path of Astronomers"—a straight line—with "Real Orbit"—a circle,—is sufficient to show it to be a paradoxical work of a very ordinary type. The author, Mr. William Sandeman, is an old correspondent of ours, whom, although we can scarcely agree with his views, we respect for his consistency, for he has been endeavouring for years to get a hearing, and failing this, at last publishes this book, which has been sent us for review, and the gist of which we think the author would like us to set before our readers.

Mr. Sandeman has no difficulty in accepting the astronomical facts that the stars gradually change their positions with reference to the Earth's orbit, and that the first Point of Aries changes its position among the stars; that is, he does not contest the *observed* phenomena of precession, but he does not agree that these result from movements of the Earth's axis and of the plane of the ecliptic. He makes the curious objection to the first of these that the movement is *so slow* as to be incredible, and substitutes for these generally accepted reasons a theory that the Sun moves in an orbit whose diameter does not exceed three billion miles in a period of about 25,800 years, and this he asserts will account for the phenomena known as precession.

This is the main point of the book. There is some attempt to pick flaws in the accepted theory, which shows incidentally that the author has read many books on astronomy, but there is no disproof, nor is any accurate proof of his own scheme given, which we must tell Mr. Sandeman, and any other person who contemplates writing a book of this kind, is the first thing to be done if his views are to gain a hearing. Let him turn his geometry into arithmetical formulæ (on p. 69 it is stated that there is a difficulty in doing this, but there should not be), and then use these formulæ to convert the places of stars—several stars widely distributed—given in one catalogue into the places as they would be at the epoch of another catalogue, and compare the result with the places of this last. The doctrine of Precession of the Equinoxes satisfies this test; if the new geometry satisfies it equally well the two doctrines might be considered, so far, on a par; and, as one of Mr. Sandeman's correspondents said—for our author has been fortunate enough to elicit opinions, considerate but scarcely encouraging, from several persons evidently well versed in the science—it is not "improbable that a geometrical arrangement could be conceived which would account for the motion of the celestial bodies, and no doubt there is much pleasure in the mental pleasure involved in its construction, but the geometry generally accepted as true will continue to hold its ground because it agrees with dynamical theories." Now Mr. Sandeman occupies a little space in making objections to the dynamical theory; but this is for the moment quite beside the question—he has not yet shown the doctrine of precession to be untrue and superseded from geometrical or arithmetical considerations.

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[*] 'The Path of the Sun; with an Exposure of the Fallacy of the Precession of the Equinoxes.' By W. Sandeman, F.C.A. Sherratt & Hughes, Manchester; Simpkin, Marshall, & Co., London.

NOTES.

Comet Notes.—The following is a continuation, for Berlin midnight, of the ephemeris of Comet *b* 1900 (Borrelly-Brooks) (*Ast. Nach.* 3663):—

		R.A.			N. Dec.				R.A.			N. Dec.	
		h	m	s	°	′			h	m	s	°	′
Oct.	3....	14	32	53	68	11	Oct.	19....	14	56	27	65	58
	7....	14	38	55	67	24		23....	15	2	16	65	46
	11....	14	44	50	66	47		27....	15	8	8	65	40
	15....	14	50	38	66	19		31....	15	14	2	65	42

The brightness at the beginning of the month is $\frac{1}{14}$ and at the end of the month $\frac{1}{30}$ of that at discovery.

Search ephemerides of Comet 1894 IV. (E. Swift) and 1884 II. (Barnard) are given in *Ast. Nach.* 3660, 3664; but the circumstances are unfavourable, and the chances of detection very slight.

A. C. D. C.

Minor Planet Notes.—Signor Millosevich has still further revised the elements of (433) Eros with the aid of recent observations, and obtains the following values after applying perturbations:—

Epoch...	1898 Aug. 2·5 Berlin M.T.		1900 Oct. 31·5 Berlin M.T.	
M	205° 21′ 41″·83		304° 24′ 40″·34	
π	121 10 51·40	1900·0	121 9 47·82	1900.0
☊	303 31 56·17		303 30 50·02	
i	10 49 35·35		10 49 38·97	
φ	12 52 14·44		12 52 40·61	
μ	2015″·26908		2015″·23324	
log *a* ...	0·1637824		0·1637875	

An ephemeris for October was given last month.

Two new planets have been discovered, both by Wolf and Schwassmann at Heidelberg; FJ on Sept. 16, FK on Sept. 22. Both are of the 13th magnitude. A. C. D. C.

A NEW FORM OF HELIOMETER.—A telescope has lately been added to the equipment of the Moscow Observatory for the especial purpose of measuring the variation of the solar diameter throughout the year and the form of its disk. The instrument in some degree is like a heliometer, for the object-glass is cut in two, along a diameter, to form two semi-circular objectives, but, unlike the heliometer, these halves have no means of motion relative to one another, but are displaced along the common diameter and fixed with such a distance between their centres that the two images of the Sun formed at the focus by the two halves of the objective are nearly in contact when the Sun is in perihelion. As the solar diameter becomes smaller the interval between the images naturally increases, and the increase can be measured with an ordinary filar micrometer. The plate in which the two half-objectives are mounted has a means of rotation, so that any diameter of the solar disk can be measured.

A NEW METHOD OF DETERMINING LATITUDE VARIATION.—It may have occurred to many that a simple and obvious effect of the motion of the pole of rotation of the Earth would be seen in the variation of the azimuth error of a transit instrument. The position of a meridian mark relative to the instrument would remain constant so far as latitude variation is concerned, but the position of the meridian on the Earth as determined by star observations would vary, since the meridian passes through the pole of rotation, which may be considered a moving point. So that the difference of azimuth error as found from a mark and from stars should show a variation with a period the same as that of the variation of latitude. The phenomenon has been investigated on these lines by Mr. J. Weeder, of the Leiden Observatory, who has published a paper entitled, "The 14-monthly Period of the Motion of the Pole of the Earth from Determinations of the Azimuth of the Meridian Marks of the Leiden Observatory from 1882 to 1896," in the Proceedings of the Amsterdam Academy for 1900, March 31. The investigation though simple in principle is complicated by various considerations, one such being the want of stability of the meridian marks, these accidental variations throughout the period having "been greater than the variation of the direction of the meridian resulting from the motion of the pole"; but after eliminating the effect of such things as this, the author evolves a formula sensibly of the same form as Dr. Chandler's, the chief differences being that the amplitude of the 430-day term is reduced in the ratio 9 : 10, and the epoch of this term is altered by about 28 days.

A DAYLIGHT METEOR.—Just before sunset on September 2, a splendid meteor was seen by observers in the North of England and Scotland, and in Wiltshire. The accounts generally state that there was a sudden flash and a bright streak as of a molten body falling in a vertical direction, leaving a column apparently of white smoke which lasted from six to thirty minutes according to different observers. Mr. Denning, writing in *Nature* of Sept. 13, says that it is not likely that the exact height of appearance and disappearance can be ascertained, but that it probably disappeared over Lancashire at a height of twenty-five miles and was directed from a radiant-point high in the northern sky. The long enduring streak or cosmic cloud was no doubt illuminated by the Sun's rays.

THE ASTRONOMISCHE GESELLSCHAFT STAR CATALOGUES.—The appearance of another zone catalogue—Leipzig I. + 10° to + 15° (the other zone allotted to this Observatory was published in 1899)—calls attention to the fact that the great work of cataloguing all the stars of magnitude 9·0 and brighter between declinations − 2° and + 80°, organized by the German Astronomical Society about the year 1870, and afterwards extended to declination − 23°, is nearing completion. The zone catalogues already published are

Albany	+ 1° to + 5°	Cambridge ..	+ 25° to + 30°
Leipzig I. ..	+ 5 ,, + 10	Bonn	+ 40 ,, + 50
Leipzig II...	+ 10 ,, + 15	Harvard	+ 50 ,, + 55
Berlin A ..	+ 15 ,, + 20	Helsingfors .	+ 55 ,, + 65
Berlin B ..	+ 20 ,, + 25	Christiania ..	+ 65 ,, + 75
		Kasan......	+ 75 ,, + 80

To complete the original scheme, there remain to be published the catalogues from Nikolajew − 2° to + 1°, Leiden + 30° to + 35°, Lund + 35° to + 40°, Dorpat + 70° to + 75°. The catalogues of the southern zones are being made by Strasburg − 2° to − 6°, Vienna (Ottakring) − 6° to − 10°, Harvard − 10° to − 14°, Washington − 14° to − 18°, Algiers − 18° to − 23°. Dr. Schorr, of Hamburg, has recently published observations which will go to form a similar catalogue of the zone + 80° to + 81°.

A Sun Pillar.—An instance of this rare meteorological phenomenon, seen by Mr. J. G. Wood from Aylstone Hill, Hereford, on September 5, is thus described by him in Symons's *Meteorological Magazine.*

> The day had been warm, with clear sky, but with haze, not sufficient, however, to obscure the outline of the Black Mountains (20 miles S.W.), or that of the Malverns (19 miles E.).
>
> Immediately after sunset (6.45) the earth shadow was very distinct and dark in the east, with crimson masses of light cumuli above it; the sky overhead perfectly clear. Over the sunset were streaks of pale golden cirrus, across which rose vertically a beam of light, of darker yellow. This continued, slightly deepening in colour, till 7 o'clock; between which time and 7.20 it passed through orange and reds to a deep crimson, and resting on the crest of Credenhill it resembled a tongue of fire rising out of the hill. Towards the end of this period two spots in the beam gradually became brighter, while the rest of the beam (actually or by contrast) became paler, and streaks of light green showed in the sky on either side. These spots, by 7.20, had developed into sharply defined mock-suns, of the same apparent diameter as the Sun itself; the lower one resting immediately on the hill, the upper one separated from the lower by the distance of two diameters, the two still connected by the beam (which also passed upwards for some distance above the upper one), and both of deep red, as of the sun setting through smoke. At 7.25 the upper mock-sun merged into the band, and at 7.30 the lower one became blurred, while the whole gradually faded; and by 7.33 nothing remained but a strong after-glow along the hills.

Dr. Wislicenus and Copernicus.—The great value, as a work of reference, of the 'Astronomischer Jahresbericht' of Dr. Wislicenus makes it desirable to point out any slip therein. In a note at p. 46 on Copernicus attention is called to one by Mr. Lynn in vol. xxii. p. 162 of the *Observatory*, correcting a statement in the 'Encyclopædia Britannica,' that Copernicus, whilst at Rome in early life, had the friendship of Regiomontanus there; the fact being that when Regiomontanus died Copernicus was only three years old, and had not yet left Thorn. Copernicus, in fact, was born in 1473, but Dr. Wislicenus gives the date as 1479, three years after instead of before the death of Regiomontanus, which took place in 1476.

The Dark Rifts in the Corona.—There is nothing new *in* the Sun. At the meeting of the R.A.S. in June 1874 Mr. Cowper Ranyard read a paper calling attention to certain dark markings on the corona as shown by the photographs taken at the Solar Eclipse of 1871 December, which he suggested were actual opaque bodies between us and the corona. The idea has been revived lately by Mr. E. W. Maunder, in a rather different form. His suggestion seems to be that the dark markings on photographs of this year's eclipse are indications of dark bodies connected in some way with the Sun; but we are unable to give his views with accuracy, for our information is derived chiefly from the daily press, where the matter has in some cases been treated rather sensationally. Sir Norman Lockyer, who was interviewed on the subject, is reported to have scouted the idea as ridiculous and so utterly absurd as to be scarcely worthy of contradiction.

THE WEATHER IN SEPTEMBER.—A somewhat variable month, cold until the 13th, with a minimum temperature as low as 40°·4 on the 4th, and a mean for the period 2° below the average. During the succeeding 13 days the weather was warm and the mean temperature 3°·2 in excess of the corresponding average, the shade-thermometer on the warmest day rising to 82°·6; but this value has been exceeded on many occasions in September of preceding years. Sunshine has been pretty equally distributed throughout the month, the amount during the first 13 days having been 74·5 hours, and during the latter part 72·7 hours. The total amount for the month was not excessive, being 33 hours less than the amount recorded in the month of September 1895. The rainfall up to the 26th was very small, amounting to 0·66 in. only, the greater part being due to one day's fall, viz. 0·52 in. on the 1st. In the month the total rainfall amounted to 1·14 in. on six days.

THE death is announced of Capt. C. Orde Browne, R.A., who will be remembered as one of the observers of the Transit of Venus 1874, when he occupied the Station at Mokattam, Egypt. His determination of the differences of longitude between Mokattam and Alexandria, and Mokattam and Suez, are links in one of the longitude chains connecting Greenwich and Madras.

THE Annual General Meeting of the British Astronomical Association will take place on Wednesday, October 31.

FROM AN OXFORD NOTE-BOOK.

THE meeting of the British Association at Bradford this year was made interesting to astronomers by the formation of an Astronomical Sub-section under the presidency of Dr. A. A. Common, who gave an excellent historical address on astronomical instruments. History may be written from many points of view, and Dr. Common chose a somewhat novel method of considering the development of the telescope by following the changes in the observer's position :—

> We have watched the astronomer, first observing from the floor level ; then mounted on a high scaffold like Sir William Herschel, Lassell, and Lord Rosse ; then, starting again from the floor level and using the early achromatic telescope ; then, as this grew in size, climbing up on observing chairs to suit the various positions of the eye-end of the telescope, as we see in Mr. Newall's

great telescope; then brought to the floor again by that excellent device of Sir Howard Grubb, the rising floor. . . . Then we have the ideal condition, as in the Equatorial Coudé at the Paris Observatory, where the observer sits comfortably sheltered, and looks down the telescope, and from this position can survey the whole of the visible heavens.

The address goes on to consider the coelostat, and the possibility of using with it a reflecting mirror of (say) 200 feet focal length, giving an image of the Moon two feet in diameter. When Dr. Common talks of such matters we know that it is no mere theoretical speculation, and we shall hope to hear of an actually constructed instrument as an addendum to this Address.

THERE was a fair sprinkling of papers to be read, of which the most important was a scheme of classifying sun-spots, by Father Cortie. He has apparently found it possible to assign all spots to a series of well-defined types, and, with perhaps a little revision of nomenclature, his scheme merits a careful trial. Mr. J. Herschel described his method of recording meteor-tracks on charts where the stars showed as bright points, easily seen in the dark. This method renders any comprehensive knowledge of the stars unnecessary, and is thus specially adapted for beginners. It has also the scientific advantage that the radiant is obtained from lines drawn in the dark, and therefore quite independently. There were some papers on the recent eclipse, and Mr. Hinks communicated some diagrams which will be useful at the approaching opposition of Eros. Altogether a very fair programme. There are so many meetings of astronomers in the regular session that an important programme in September is not to be expected; but the new departure was quite sufficiently successful to merit a second trial.

THE astronomical sub-section met on two days only of the six, so that on the other four we were able to attend the regular meetings of Section A, and especially to hear the brilliant address of Dr. Larmor on molecules and the æther. This address gave us much to think about at leisure, as Prof. Carey Foster remarked at the close of a most interesting meeting. A single quotation will indicate its searching character:—

> A very large amount of effort has been expended by Maxwell, Helmholtz, Heaviside, Hertz, and other authorities in the attempt to express the mechanical phenomena of electrical action in terms of a transmitting stress (in the medium). . . . According to the views here developed, that ideal is an impossible one; if this could be established to general satisfaction the field of theoretical discussion would be much simplified.

This threatened destruction of the ideas which we inh
Faraday and Maxwell is rather distressing to some of
not been able to follow closely the recent progress

ideas; but Dr. Larmor was, throughout his Address, ruthless in claiming the destruction of scaffolding which had served its purpose—and this purpose he defined as the attainment of a mathematical formula. When by slow degrees we had climbed up to a fundamental equation the ladder might be kicked away, the equation pure and simple was now all we wanted. Prof. Fitzgerald rose in rebellion after the Address and avowed his preference for "plum-pudding" physics—for brass wheels and bands (one wicked reporter wrote "brass bands") rather than an equation with integrals in it. But even those who felt soothed by this counterblast knew that the Address was really good for them, and they must try and digest it; and really when I read it again, as I have just done, it is a *very* good address—the right adjective is not easy to find; I was going to say "scholarly." Before leaving it, I would incidentally call attention to the comparison of Helmholtz's recognition of the permanent state of motion constituting a vortex ring, and the consequent discovery of the types of permanent motion in a fluid system, with G. W. Hill's introduction of the idea of permanent orbits into astronomy.

THERE was a good, though rather long "Red Lions" dinner, but the proceedings are regarded as private. This privacy, however, may be considered to lapse with years, and there can be no harm in recording here a remark which I am told was made by W. K. Clifford at the dinner in Bradford 27 years ago. He said that he had been puzzled what was meant by an "atom," and had had the curiosity to look it up in a work of reference. He found the entry:

"AT 'EM, Up Guards and."—See Wellington, Duke of.

This recalls the index to the biography of Lord Brougham (?), in which one of the references was to Lord B.'s "great mind"; and on turning to the page indexed, the entry was found to be that "Lord B. had a great mind to commit a certain witness for contempt of court."

THE scheme for a general subject-index of scientific literature has been several times referred to in these notes, rather disparagingly, and in the course of many conversations I have not come across any enthusiasm for the project. But it seems to be going through, for the substantial reason that the money is forthcoming in some way or other; and if this is the case we must do our best to give it a fair trial. There is no doubt of the usefulness of such an index if it can be provided; the chief objection to it is the enormous expense and trouble for a comparatively slight return. But if the question of expense is settled by those who make it their concern, we should be worse than ungracious to refuse all the help we can give. At the present moment the state of affairs seems to be that a scheme of classification has been drafted and circulated for criticism. Perhaps the most useful form of criticism is experimental, and by way of testing the scheme I ran through a number of titles the other day to see how the astronomical portion of the scheme would work. The following are some of the titles which I found it difficult to assign to their proper headings—that is to say, to assign *definitely*, with fair confidence that some one else would do it in the same way; for this should be an essential feature of the classification unless the work is all to be done by one man:—

Transmission of sunlight through the Earth's atmosphere.
Atmospheres upon planets and satellites.
Occultations of stars by planets.
Time determinations by observations out of the meridian.
On the mechanical conditions of a swarm of meteorites and on theories of cosmogony.
"Astronomical Photography."
Determination of the errors of the Cape réseau.
On the physical constitution of the Moon.

Most of these would no doubt appear under several headings: but this is not a complete solution of the difficulty, unless the series of headings would be selected in the same way by everybody. One minor detail peculiar to astronomy may be noticed: we must settle how to catalogue individual stars. The R.A. changes unless we adopt a definite epoch (1900·0?), and there are objections to using the constellations for cataloguing purposes.

The third edition of that most useful work, Lancaster's *Liste Générale* of astronomers and observatories (I am away from references and cannot give the exact title) is getting rather out of date. Astronomers will be glad to learn that M. Lancaster, in response to numerous requests, intends to spend his leisure in the coming winter in preparing a new edition. Doubtless he would be glad of corrections of any kind. His address is A. Lancaster, Uccle, Belgium.

The following from a correspondent:—"There seems to be a tendency among English-speaking astronomers to send letters to Scandinavian observatories under the address 'Sternwarte.' As this word is only intelligible to those who have learnt German, it happens now and then (presumably with the advent of a new postman) that letters thus inscribed only reach the observatory by a circuitous route: while the word 'Observatorium,' either in original Latin or in one of its derived forms, is intelligible to Scandinavian postmen.

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The Zodiacal Light.

As the Zodiacal Light, which has been brought into notice by members of recent eclipse parties, is a little-known phenomenon, it is thought advisable to publish a concise history of the subject. The following two papers have therefore been chosen as representing our knowledge of the matter up to the year 1855.

The first paper is a portion of an article by M. E. Lefébure in *Ciel et Terre*, 1894, April. The second is in reality a review, by Professor C. E. Brame, of the work done in this connection by the "United States Japan Expedition."

The Zodiacal Light according to the Ancients.

According to Gruson and Brugsch the Egyptians were acquainted with, and even worshipped, the zodiacal light from the very earliest times, as a phenomenon visible throughout the East before sunrise and after sunset. It was described as a glowing sheaf or luminous pyramid perpendicular to the horizon in summer, and inclined more or less during the winter. Indeed the Egyptians represented the zodiacal light under the form of a triangle which sometimes stood upright and at other times was inclined. M. Brugsch further remarks that to continue this knowledge of the Egyptians we must come to much more recent times viz., to the year 1660.

Nevertheless, it appears very certain that this phenomenon had been observed by the Greeks long before the Christian Era, although neither M. Gruson nor M. Brugsch have mentioned it, and it will, perhaps, be useful to briefly show this. Diodorus in his description of Arabia says "the Sun appeared to come up from the sea like a burning coal which throws out sparks, that it did not at all appear, as to us, under the form of a disc, but that it rose up from the horizon as a column of which the chapiter was a little crushed." Diodorus further cites Agatharcide of Cnidus, author of a history of Asia, and from whom the preceding is certainly borrowed, as follows:—"at his setting, after having disappeared, he seems to light the Earth with new rays." The last French translator of Diodorus, M. Hoefer, does not dispute this, and has added the following note to the text:—"A column of fire rose from the horizon, a sheaf of light throwing its sparks like a glowing coal, &c., in a word, several of these phenomena mentioned by the author seem to apply well to certain effects of the zodiacal light."

Strabo, who gathered much from the works of Artimidorus and Erastosthenes on the subject of Arabia, says nothing of it, neither does Herodotus. The same phenomena had, however, been remarked in quite another region. Certain Greek philosophers, whose opinions Lucretius has given us, pretend that on each day a new sun was born from a condensation of glowing clouds, and, says this poet:—

> Quod genus Idæis fama est e montibus altis
> Dispersos ignes orienti lumine cerni,
> Inde coire globum quasi in unum et conficere orbem. *

A little later Diodorus writes of this fact, in speaking of Mount Ida, in a less summary manner:—"This same mountain offers a strange phenomenon, at the moment when the Dog (Sirius) rises to the summit of Ida, the air which envelops the summit being so calm that it is apparently above the region of winds, and, the night still remaining, the Sun is perceived rising with projecting rays, not in the form of a disc, but of a flame whose rays spread in all directions. After a while these separate rays re-unite in a single column 90 mètres in dimension, and then the day commences, the Sun appears with the usual luminous disc." Finally, according to Diodorus, Pomponius Mela says the same thing.

These writers borrowed their information from some Greek works anterior to their time, probably from Xenophon, who lived in the sixth century before the Christian Era. Agartharidus lived in the second century B.C.

Before proceeding with the second paper, it may be of interest to interpolate a few further allusions to this phenomenon. Thus Nicephorus noted it about 410 A.D., and remarks that it lasts from about the middle of summer to the end of autumn. In the Koran it is called the "False Aurora." In 1630 Descartes observed it. Cassini commenced his studies on the Zodiacal Light in 1683. Euler adopted Cassini's theory. Laplace, too, thought it might be an atmosphere of the Sun, turning with him, and stretching as far as Mercury. Hooke (1705) considered it as matter disposed around our globe, a theory regarded for a long time with much favour, and afterwards adopted by Barnard (*American Journal of Science,* xxi. 1856). The idea of an

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* De Natura Rerum, v. 662-4; *cf.* 730.

"effluve repoussé" by the Sun, following the prolongation of the radius vector, like the tails of comets, is in a note by Brorsen (*A. N.* xlix. 1859), and this germ has been developed by Houzeau in the early numbers of *Ciel et Terre.*

The Zodiacal Light*.

The purpose of this contribution is to draw attention to a phenomenon which has received too little notice, and has been strangely neglected by astronomers, but which, in fact, if the conclusions of the author of the work under review are correct, is to the inhabitants of the Earth one which emanates from a very near and remarkable cosmical body.

The third volume of "The United States Japan Expedition" records a series of observations on the zodiacal light, which were made by Rev. George Jones, A.M., Chaplain in the United States Navy, from April 2, 1853, to April 22, 1855, during which time he accompanied that expedition sent out by the United States Government, under the command of Commodore M. C. Perry. It also contains the observations and theories of other astronomers, particularly those made by the celebrated Dominicus Cassini, a distinguished *savant* of the seventeenth century, attached to the Royal Observatory at Paris, who was the greatest cosmologist of that age.

The deductions of our author are :—1. That the zodiacal light is emitted by a nebulous ring, with the Earth for its centre; that is, there is a ring of nebulous matter around the Earth; 2. That 60° is the full width of the stronger light, and 90° its full width including the diffused light; 3. That it is on both sides of the ecliptic; 4. That it may be seen in some latitudes under favourable circumstances, forming a continuous arch across the heavens; 5. That it must rotate on a common centre, and seems full of commotion within itself; 6. From observations made then and afterward at Quito, in South America, that it is not entirely dependent on light reflected from the Sun, but must shine partly by its own light.

These conclusions are given to the public with much diffidence, and with strong arguments to support them, based on a large accumulation of facts ascertained by the most careful observation.

Our Chaplain became intensely interested in this investigation, and has recorded three hundred and twenty-eight observations on stereotyped star-charts, published with the volume. Sometimes, when unable from sickness to go upon deck, he was carried in his cot, that he might have an opportunity of noticing this intensely interesting phenomenon. As the voyage was one which circumnavigated the globe, he had a most favourable opportunity to prosecute his examination; and his recorded statement embraces a most minute and faithful account of his observations in nearly every longitude, and over a tract extending from 41° 49′ north to 53° 48′ south latitude.

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* Review by Prof. Brame in the *Popular Science Monthly*, 1877, October.

After the return of the expedition he sought the earliest occasion to go to Quito, in South America, which is directly on the equator, where he saw the light extending in one continuous arch across the heavens. During his residence there he published several articles in *Silliman's Journal* on "The Zodiacal Light," which gave additional interest to the subject.

If anything since then of importance has appeared in any scientific journal on this subject, we have not met with it. Having for over twenty years considered it, and being anxious that more information and more confirmed conclusions should be arrived at concerning it, we invite the attention of the learned and observing to the consideration of this subject. We believe that there is no physical truth which stands isolated, but every new discovery in physical science opens the way for other truths and discoveries. If naturalists can spend days and weeks in examining and commenting on a new discovery in botany or zoology; if foreign countries are visited, forests and mountains traversed, to bring to light some hitherto unknown plant; if it is a triumph for a naturalist to discover a new species of microscopic animalcula—surely it is not time wasted to direct attention to a cosmical body so interesting as the zodiacal light.

Astronomical science is full of attractive interest to every lover of Nature. It is not astonishing that the study of the heavens is the oldest natural science. At an early period of the world's history the human eye and intellect were directed, with absorbing interest, to the azure arch above us, amid whose vast expanse orbs of grandeur are unceasingly running their wonderful courses. The stars, beaming with inextinguishable brilliancy, are known to be oceans of flames and centres of worlds, though apparently but points of light. The planets were known to the ancients to be more identified with our world than other stellar bodies. Traversing space with inestimable velocity and performing their revolutions with unvaried regularity, they have long been known as part of our solar system. It seems strange that a cosmical body so near the Earth as the zodiacal light should have received comparatively so little attention.

As many who read this article have never seen this light, it is necessary that it be described.

It is defined, in the work under review, to be "a brightness that appears in the western sky after sunset, and in the east before sunrise; following nearly or quite the line of the ecliptic in the heavens, and stretching upward to various elevations according to the season of the year." There is a slight objection to this definition, by which inexperienced observers may be led astray. It is spoken of as appearing after sunset, by which some would be led to suppose that it is visible immediately after sunset; whereas it is never to be seen until after the night has fully set in, and the

Sun's rays are some distance below the horizon. Its varied elevation, indeed its appearance, is also dependent upon the latitude of the observer as well as the season, so much so, that in very high latitudes it is but seldom seen to advantage. It has also been seen at almost every hour of the night, but is usually more distinctly seen in the temperate zones, when observed between dark and nine o'clock, as after that hour its light frequently becomes dim and diffuse.

> It appears to best advantage when the ecliptic makes its highest angle with the spectator's horizon, at which time, in moderate latitudes, it reaches to the zenith or beyond it, having near the horizon a striking brilliancy, and thence fading upward, mostly by imperceptible degrees, till at its vertex it can be made out only by a careful and experienced eye. As the seasons advance, when the ecliptic is declining gradually toward the horizon, the zodiacal light fades away till it is perhaps entirely lost to view, or can be seen only by those who have followed it in its changes, night after night, and are thus able, by familiar acquaintance, to detect and trace its dim markings in the sky.

Humboldt, in 'The Cosmos,' vol. i., remarks :—

> Those who have been for many years in the zone of palms must retain a pleasing impression of the mild radiance with which the zodiacal light, shooting pyramidically upward, illumines a part of the uniform length of tropical nights. I have seen it shine with an intensity of light equal to the milky-way in Sagittarius, and that not only in the rare and dry atmosphere of the summit of the Andes, at an elevation of from 13,000 to 15,000 feet, but even on the boundless grassy plains, the llanos of Venezuela, and on the sea-shore beneath the ever-clear sky of Cumana.

Prof. Olmsted, in his 'Astronomy,' describes it as follows :—

> Its form is that of a luminous pyramid, having its base toward the Sun. It reaches to an immense distance from the Sun, sometimes even beyond the orbit of the Earth. It is brighter in the parts nearest the Sun than in those that are more remote, and terminates in an obtuse apex, its light fading away by insensible gradations until it becomes too feeble for distinct vision. Its aspects vary very much with the season of the year.

The crepuscles, or streaks of light from the Sun, must not be mistaken for the zodiacal light; the former are sometimes visible between twilight and dark, the latter not until the shades of night have fully set in. Neither must the milky-way be mistaken for it by inexperienced observers, as the zodiacal light has a warm yellowish tinge unlike the cold white light of the milky-way.

Having endeavoured to open the way for a consideration of the subject, we will now proceed to give a history of former observations.

There is no mention of any appearance of this light by very early writers. There is mention of Arcturus and the bands of Orion in the book of Job, and the constellations of the zodiac were assigned names at a very early date. The zodiacal belt was in use among the ancient Egyptians and Hindoos. If this light had been visible, it is highly probable some ancient writer would have spoken of it. Our author observes :—

> It is scarcely probable that a phenomenon so striking in southern latitudes

should have escaped the attention of early astronomers in those countries, but we meet with nothing in their works (referring to it) of a fully definite and reliable character.

He is, however, of the opinion that it may have been overlooked or mistaken for the crepuscle at early dawn or twilight.

It has been supposed that Pliny, who wrote in the first century of the Christian era, alludes to it under the name of *trabes* or the *δοκους* of the Greeks, but Humboldt thinks that Pliny refers to another matter.

Ammonius, in his life of Charlemagne, A.D. 807, mentions an appearance somewhat like the zodiacal light, but there was no reliable notice of it before it was described in Childrey's 'Britannia Baconica' in 1661, which gives a brief description of its appearance and shape. The reference to it may be found in that work, at page 183.

[To be continued.]

Changes in the Stellar Heavens.

[Continued from p. 374.]

Perseus.—In describing this constellation, Al-Sufi speaks of the well-known double cluster, known as χ Persei, as follows:—"La 1^{re} est le petit Nuage qui se trouve sur la cuisse de la Chamelle, et que nous avons mentionné à la description de la Femme assise; il est placé sur la main droite." In describing the 12th star, which is β Persei, or Algol, the famous variable star, he says "La 12^{e} est la brillante d'un éclat rouge et des moindres de la deuxième grandeur; Ptolémée la dit absolument de deuxième. Elle se trouve dans la Tete d'*al-gul*, s'inclinant environ deux coudées de la 11^{e} [κ Persei] vers le sud, et c'est celle que l'on marque sur l'astrolabe et que l'on nomme *râs al-gûl*, la Tete d'Al-gûl." Al-Sufi's description of Algol as a red star is very interesting and remarkable, as it is now quite white. That it was red in Al-Sufi's time seems certain, for in describing the 23rd star of Perseus [ϵ Persei] he says "Avec la 7^{e} (α Persei) qui est la brillant de côté droit, et *la brillante étoile rouge* qui se trouve dans *la tête d'al-gûl*, elle forme un large triangle." [The italics are mine.] The magnitude assigned to Algol by Al-Sufi [2–3] is quite correct when at its normal brightness. It was measured 2·31 at Harvard and 2·40 at Oxford. Al-Sufi rated ζ and o Persei of the same mag. [3–4], and so did Ptolemy, whereas Argelander, Heis, and the Harvard measures agree in making ζ about one magnitude brighter than o, ζ being 3 (3·10 Harvard) and o 4 (4·01 Harvard). This result is very interesting, as the stars are close together and their present great difference in brightness is noticeable at a glance. Al-Sufi's description of these stars is as follows:—"La 25^{e} (o) est la précédente des deux qui sont dans la jambe gauche, et se trouve dans le talon. Elle est de moindres de la troisième grandeur. La 26^e (ζ) est la suivante entres lesquelles, à vue d'œil, il y a environ une coudée. *Elle est aussi des moindres de la troisième grandeur*, et ces deux étoiles sont les plus proches des Pléiades il n'y a pas d'étoile entre celles-là et les Pléiades." [The italics are mine.] It seems probable that o has diminished in brightness, for the present magnitude of ζ compares very well with what Al-Sufi made it. It was measured 3·10 at Harvard, 3·07 at Oxford, and 3·14 at Potsdam. The star 15 Persei, which Ptolemy called faint, was rated 5th magnitude by Al-Sufi, 5–4 by Argelander and Heis, and it was measured 4·44 at Harvard and 4·54 at Potsdam—probably another case of brightening. Ptolemy and Al-Sufi agree in making θ Persei considerably brighter than τ, but τ is now slightly the brighter of the two. τ has probably increased.

With reference to the comparatively blank region lying between Auriga, Ursa Major, and Cassiopeia, Al-Sufi says:—"Dans la partie du ciel alentour de cette figure (Auriga) de la tête du Grand Ours, de l'étoile *al-djudaï* et de la Femme assise (Cassiopeia) ce qui est un portion du ciel ressemblant à un désert, il n'y a aucune étoile brillant."

Auriga.—The star η is now considerably brighter than ζ, although rated of the same magnitude (4) by Ptolemy and Al-Sufi.

Ophiuchus.—The star σ Ophiuchi, which lies about 4½° preceding β Ophiuchi, seems to have brightened since Al-Sufi's time. Al-Sufi says that it is "entres les épaules" of the ancient figure, and "de la sixième grandeur." But it was rated 5th mag. by Argelander, 4–5 by Heis, and it was measured 4·42 at Harvard and 4·45 at Oxford. It has been suspected of variation. The star σ Serpentis—which Al-Sufi includes in Ophiuchus—would also seem to have brightened, for Sufi made it 6th magnitude, while Argelander and Heis rated it 5. It was measured 4·76 at Harvard and 4·92 at Oxford.

Sagitta.—The star δ has probably increased in brightness since Al-Sufi's time. It was rated 5th magnitude by Ptolemy and Al-Sufi, but 4th magnitude by Argelander and Heis. It was measured 3·72 at Harvard, 3·83 at Oxford, and 3·94 at Potsdam.

Aquila.—Schjellerup identifies Al-Sufi's 5th star of this constellation with ν Aquilæ, but this is an error; Al-Sufi's star is clearly γ Aquilæ. Ptolemy rated μ and σ of the 5th magnitude, and Al-Sufi calls them 6, but at present μ is about 4·7 and σ about 5. μ is a suspected variable. Curiously enough, Al-Sufi does not mention ϵ Aquilæ, which lies close to his 9th star ζ. ϵ was suspected of variability by Sir William Herschel, who found it in 1784 "not much larger than 11, 18, or 19," which are about 5th magnitude. Yet ϵ was rated 4th magnitude by Argelander and Heis, and was measured 4·10 at Harvard, 3·86 at Oxford, and 4·28 at Potsdam. It has probably brightened considerably since Al-Sufi's time. Al-Sufi refers to the cluster of small stars surrounding 4 and 5 Vulpeculæ as a "petit Nuage."

Pegasus.—Al-Sufi rates the stars μ and λ of this constellation as 4–3, and says that Ptolemy made them 4 magnitude. Here Ptolemy was right—at least according to modern estimates—although he is usually less accurate than Al-Sufi.

Andromeda.—It is curious that Al-Sufi does not mention either ξ or ω Andromedæ, which lie close together between his 20th star (φ) and his 22nd star (χ), for they are both, now at least, slightly brighter than χ. ξ has been suspected of variation. It is interesting to find that Al-Sufi speaks of the great nebula in Andromeda as "petit Nuage située très près de la 14^e" (ν Andromedæ), as it was unknown in Europe until 1612, when it was observed by Simon Marius, who described it as resembling "a candle shining through horn."

Triangulum.—The star γ Trianguli seems to have diminished in brightness since ancient times. It was rated 3rd magnitude by Ptolemy, 3–4 by Al-Sufi, 4–5 by Argelander and Heis, and it was measured 4·15 at Harvard, 4·35 at Oxford, and 4·32 at Potsdam, or about one magnitude fainter than Ali-Sufi made it.

Aries.—Al-Sufi says that the stars β and γ of this constellation are both of the 3rd magnitude, γ being slightly the fainter of the two; but at present γ is more than a magnitude less than β, which seems to show that γ has diminished in brightness, β being still about the 3rd magnitude. The spectrum of γ Arietis is of the first or Sirian type. Ali-Sufi mentions the small star κ Arietis, which lies closely south of α, and which Ptolemy did not record. The fact of Al-Sufi having seen this small star, which modern measures show to be below the 5th magnitude, is strong evidence of his keen eyesight.

Taurus.—Flamsteed 47 Tauri, which lies closely north preceding μ Tauri (Al-Sufi's 7th star), is not mentioned by Al-Sufi, although it is, at present at least, of the 5th magnitude and distinctly visible to the naked eye. It seems curious that Al-Sufi should have noticed κ Arietis and overlooked 47 Tauri, if the latter was as bright in his time as it is at present. Schjellerup does not identify Al-Sufi's stars in Taurus numbered 29, 30, 31, and 32, but it is evident from Al-Sufi's description that they are the four brightest stars in the Pleiades, 32 being probably η (Alcyone). In fact, Al-Sufi says with reference to these stars: "Il est vrai que les étoiles des Pléiades surpassent bien les quatre mentionées ci-dessus, mais je me borne à ces mêmes quatre, parce qu'elles sont très-près l'un de l'autre et les plus grandes; c'est pourquoi nous les avons mentionée en négligeant les autres."

Gemini.—The star γ Geminorum was rated 3rd magnitude by Ptolemy and Al-Sufi, or equal to δ Geminorum; but γ is now of the 2nd magnitude, and its great superiority in brightness to δ is noticeable at a glance. This is one of the most remarkable cases I have met with in Al-Sufi's work.

Cancer.—Closely following ι Cancri (Al-Sufi's 7th star) are a number of stars of the 5th and 6th magnitudes (ρ^1, ρ^2, σ^1, σ^2, σ^3, &c.),

which Al-Sufi does not mention, although some of them are now as bright as others he speaks of.

Leo.—The remarkable diminution of brightness in β Leonis since Al-Sufi's time has been considered in my paper on "Some suspected Variable Stars" (*Knowledge*, Aug. 1899). That it was a bright star of the 1st magnitude in former times is fully proved by the observations of Ptolemy, Al-Sufi, and Tycho Brahé. These were careful and accurate observers, and could not have been mistaken about a star of the 1st magnitude. β Leonis is now fainter than an average star of the 2nd magnitude, and there can be no reasonable doubt that it has faded considerably since the 10th century. It has a spectrum of the Sirian type.

With reference to the star ξ Leonis, which Ptolemy and Al-Sufi rated 6th magnitude, but modern photometric measures make about 5·2, Pigott remarks *: "Montanari says this star was hardly visible in 1693. I found it constantly in 1783, 1784, and 1785 of the same brightness, being of the 5th mag.; less than A, π and if any difference rather brighter than h and ω Leonis. Tycho Brahé, Flamsteed, Mayer, Bradley, &c. make it of the 4th mag." Pigott's observations would make ξ about 5·4 m. in the years he observed it.

Virgo.—Al-Sufi rates both η and γ of the 3rd magnitude, but modern photometric measures make γ more than a magnitude brighter than η. γ is a suspected variable. Al-Sufi says that (contrary to Ptolemy) ζ Virginis is less than ϵ, an observation which is confirmed by modern measures, and another proof of the great accuracy of Al-Sufi's observations. Al-Sufi speaks of the 5th star of Ptolemy's "externs"—61 Virginis—as "une étoile double de la cinquième grandeur"; but there is at present no star near it visible to the naked eye, the nearest being 63 Virginis (about 5½ mag.), which lies about 1° north following.

Libra.—With reference to the 1st star of this constellation—α Libræ—Al-Sufi says: "Elle est de grandes de la troisième grandeur. Ptolémée la dit de la deuxième, mais elle n'est pas plus brillante que celle de la tête du Psylle [α Ophiuchi] ou que celle à l'extrémité de la queue du petit Ours [Polaris] et il n'est pas possible de mettre cette étoile au rang de celle du corps du Scorpion" [Antares]. I quote this remark, which agrees with modern observations, to show that Al-Sufi actually compared the stars with each other in brightness as modern observers do.

Scorpio.—Al-Sufi describes θ Scorpii as a star of the 3rd magnitude; but as it is now of the 2nd, it has probably brightened since Al-Sufi's time. He also rated λ Scorpii 3rd magnitude, but it is now about 1¾. He made λ and κ both 3rd magnitude, but κ is now nearly one magnitude less than λ. Al-Sufi made no mistakes of this sort, and λ has probably increased in brightness since Al-Sufi's time. Schjellerup does not identify Al-Sufi's 15th star, and makes the 14th ζ Scorpii. From Al-Sufi's description, however, I believe the 15th star to be ζ, and the 14th a nebula closely north of ζ, which I have often seen in India as a hazy star of about 4½ magnitude. This object and ζ Scorpii agree well in position with Al-Sufi's description.

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* 'Philosophical Transactions Royal Society,' 1786.

Aquarius.—Al-Sufi makes the stars h (83), ϕ, and χ all the same magnitude, 4–5; but h is now only 5·45. Schjellerup does not identify the 27th and 28th stars of Al-Sufi; but these are clearly ψ^1 and ψ^2 Aquarii, the 29th star being ψ^3, as Schjellerup says. Al-Sufi rates the stars c_1 (Fl. 86), c_2 (Fl. 88), and c_3 (Fl. 89) as all of the 4th magnitude, but modern measures show that c_2 (the northern of the three) is more than a magnitude brighter than c_3 (the middle one of the three) J. E. Gore.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

On the Resolution of Double-Stars.

Gentlemen,—

I cannot agree with some of Dr. Johnstone Stoney's conclusions in the October number of the *Observatory*; and the cause of our difference lies in his second sentence, "The image of a star consists of a spurious disk and diffraction-rings. *We are only concerned with the disk.*"

With the full aperture of a telescope more than 92 per cent. of the light goes to form the central disk, and it is justifiable in that case to neglect the rings. But when the object-glass is covered by diaphragms such as Dr. Stoney recommends, the rings become of importance and must concern the practical astronomer. It is, of course, well known and pointed out in several treatises on "Optics," beginning with Sir John Herschel's article in the 'Encyclop. Metropolitana,' that the central disk may be diminished in size by proper diaphragms; but at the same time the luminous intensity of the rings increases until they cannot any longer be neglected in discussing the resolving power.

[If I may be allowed a parenthesis to make a remark which I do not like to relegate to a footnote, I should like to draw renewed attention to Sir John Herschel's article. It seems to me to be remarkable that, in spite of the enormous strides of science, Sir John Herschel's treatise, published over 70 years ago, should still, just as much as his 'Outlines of Astronomy,' keep its place as a standard work. I possess few books which I consult more frequently, and none in which I more often find what I want. Most modern books only give you what you know already.]

In the extreme case that the disk is contracted to half its original diameter or to a quarter of its original surface, the light forming it will only amount to about 18 per cent. of the total light. Yet Dr. Stoney considers the remaining 82 per cent. of no account in discussing the definition obtained.

My principal objection relates to his "Postscript," referring to objects of visible size, written perhaps in a hurry and without due consideration. The drawings which Sir John Herschel gives of the appearance seen when the central portion of the object-glass is covered by a circular diaphragm, having for diameter about ·9 of the full diameter, are sufficient to show that the size of the central disk is in this case of comparatively little importance, and that the illumination and extension of the rings is quite sufficient to invalidate Dr. Stoney's conclusion that covering up his object-glass by a diaphragm equal in diameter to 139/140 of the full aperture would, in the case of the Sun, "bring more detail into view than has ever yet been seen."

With Dr. Stoney's application to the resolution of double-stars astronomers will be better able to deal than myself; but I should like to offer two remarks. In the first place, the resolution of two stars must to a great extent depend on whether they are of about equal brightness or whether a bright star has a feeble companion. In the latter case Dr. Stoney's advice as to the best size of a diaphragm to bring out the companion seems to me to be peculiarly bad, for covering up the lens as he shows in his two last diagrams would bring the second star just behind the first diffraction band (not a ring in this case) of the first star. This, I always understood, astronomers were particularly anxious to avoid. My second remark is addressed to astronomers who have not gone into the theory of the question and wish to put Dr. Stoney's conclusion to a practical test. The latter takes two stars to be resolved when their star-disks touch other, and for his purpose he is of course quite justified in doing so; but experience shows that stars are recognized as double-stars in practice when the centre of one disk comes over the first dark ring, so that the stars need only be half as far apart as in Dr. Stoney's test. The consequence is, that with *full* aperture the object-glass would probably resolve double-stars as near together as those taken in his extreme case, when most of the objective is covered up. My point is that the stars he wishes to resolve would in practice be resolved with full aperture, but not probably with his diaphragms, as they are unfavourably placed, though he could probably resolve stars which are nearer together. Without wishing to invalidate his theory, I contend that his examples are so chosen that the diaphragms do harm instead of good. That by covering up the central portion of the telescope lens, resolving-power may, in certain cases, be increased, I do not wish to deny; on the contrary, it would be a surprise to me if this fact should prove to be new to astronomers.

Yours faithfully,
Arthur Schuster.

Manchester, 1900, Oct. 9.

Through the courtesy of the Editors, I have been allowed to see a proof of the foregoing letter, and I can endorse what my friend Professor Schuster says in it of the value of Sir John Herschel's article on Light in the 'Encyclopædia Metropolitana,' and his article on Sound seems to merit equal commendation.

Practical Astronomers are so familiar with many of the matters to which Professor Schuster calls attention, that when I consented to the publication of my note, which had been written for a special purpose and ostensibly deals with only a portion of a large subject, I assumed that these matters would be known to any person likely to read the note.

But there are two places where Professor Schuster has misunderstood my meaning—in one place perhaps because I had not taken sufficient care in expressing it, viz., where I said "We are only concerned with the disk." What I here intended was to indicate that the note which I was then writing would treat only of the disk; but I see that the words I used are capable also of the different interpretation put upon them by Professor Schuster.

It is not so easy to see how he missed my meaning in another place. He says Dr. Stoney "takes two stars to be resolved when their star-disks touch each other." I have nowhere made this erroneous statement, and even carefully guarded against its being by any reader supposed that this was what was meant, by using the words *separate* and *distinct*, by defining these terms, and by italicising them to prevent their being confounded with any others. I also endeavoured to make a mistake of the kind impossible by pointing out that the spurious disk as I defined it had about double the diameter of its nucleus or bright part.

A double star is *resolved* when on a good night it can be clearly seen that it comes from two objects; and I have seen stars so resolved, under unusually favourable circumstances, when the interval between the centres of their images was considerably less than the interval mentioned by Professor Schuster—when it was probably not more than one-third of d, the diameter of the first dark ring. All matters relating to resolution, and to the elongated image which is often seen when the stars are too close for resolution, bear a definite relation to d of such a kind that if d is known they all become known. Accordingly, in the brief note on which Professor Schuster comments, I thought it sufficient to treat only of d, as the note was written for persons who knew all these things.

On another point I think Professor Schuster's letter needs correction, viz., as regards the effect of the light in the rings surrounding the spurious disks. This light, with an object like the Moon, would, I think, not produce in the telescope the effect he supposes. The light of the rings introduces into the image what we may call *foreign* light, meaning by this term other light than that which produces an image of anything upon the object, which latter is exclusively formed by the spurious disks. Experiments with the microscope seem conclusively to establish the fact that where, as upon the Moon or Mars, the detail upon the object is irregularly disposed, and does not arise from a periodic repetition or quasi-periodic repetition of structure upon the object, in such cases the light of the rings surrounding the several spurious disks will not prevent the true image being reasonably well seen. In fact the "foreign" light, instead of producing definite markings of the kind known to microscopists as "intercostal markings" is apt then to take the form of somewhat shapeless flickerings of light, easily distinguished from the true image and but little obscuring it.

G. JOHNSTONE STONEY.

Position of the Red Spot on Jupiter in 1899.

GENTLEMEN,—

Mr. A. S. Williams has returned to the discussion of this question in your last number. He insists that the red spot *preceded* the hollow in the great southern equatorial belt, though Antoniadi, Flammarion, Gledhill, Phillips, myself, and others independently found that the spot *followed* the hollow.

Mr. Williams quotes Prof. Hough as saying that probably the spot "has not changed materially in size during the past 20 years." But this statement does not necessarily affect the point at issue, for Prof. Hough says nothing as to the spot being placed either to the east or west of the hollow. And apart from this Prof. Hough's measures in 1897 and 1898 were few and discordant. On 1898 April 9 he found the length of the spot 10″·30, and on May 8 12″·45, and he says his results should be regarded as approximate.

I am surprised that Mr. Williams should have quoted Prof. Hough's observations at all, seeing that they contradict his own. In *Ast. Nach.* 3596 Mr. Williams says he found from observations on May 3 and June 11, 1899, that the spot had a mean length of 31°·7, whereas the corresponding value from his observations in 1887 was 34°·7, "so that the spot would appear to be a little shorter at present than it was in 1887." Prof. Hough made the length of the spot about 37° in 1897–98, so that he must have seen 5°·3 of it which were invisible to Mr. Williams! Yet the latter avers that he saw the whole of the spot, while other observers only saw the darker part of it!

On 1899, September 18, Mr. Williams wrote as follows:—"With reference to the position of the red spot relative to the hollow, in making my observations I usually purposely avoided direct comparison between the two in order that the times of the former might be independent of the latter. Hence I have few notes recorded bearing directly upon the subject. It was only on June 16 that the spot appeared to decidedly precede the hollow. Possibly it may have been this observation which gave rise to the impression in my mind that the spot preceded the hollow; but, judging from my notes *collectively*, I should think the spot might be said to exactly coincide with the hollow in May and the first half of June."

It will here be noted that Mr. Williams acknowledges that he *purposely avoided comparing the relative positions of the red spot and hollow*, and that he should think *the two objects exactly coincided*! After these admissions it is astonishing to find him stoutly maintaining the idea that the spot preceded the hollow, and the more particularly so when such a conclusion is directly negatived by several observers whose sight is probably as good as his own, while their telescopes are better.

In *Ast. Nach.* 3596, Mr. Williams says his observations in 1899 were more "discordant than usual, and that this was due, not so much to the faintness of the spot as to the poor seeing nearly always prevailing." In a letter to me he also said, in June 1899, that "the generally poor seeing has prevented my doing anything with the more delicate markings." After this, we can readily understand that he might err as to the exact position of an object so faint as the red spot. There is very little doubt that from the causes assigned his transits were a little too early, and thus he concluded the spot must precede the hollow, though he purposely avoided direct comparison. Other observers who differ from him made a special point of noting the relative places of the two objects. The length of the spot, whether invariable, as suggested by Prof. Hough's measures, or shortening, as indicated by Mr. Williams's results, does not necessarily prove anything, for the hollow itself is subject to alteration. Capt. Molesworth, in the Memoirs of the B. A. A., vol. viii. p. 74, says that "the breadth of the red spot bay from shoulder to shoulder has decidedly increased. It was 37° in 1898 June, 35°·5 in 1899 June, and 42°·5 in 1899 September." I am, Gentlemen,

Yours faithfully,
W. F. Denning.

Bishopston, Bristol,
1900, Oct. 5.

[We are glad to have had the opportunity of publishing the views of two such experienced observers on this important point. We hope that no injustice is done to either of them by saying that this must be the last letter on the subject.—Eds.]

Early Observations of the Leonids.

Gentlemen,—

We are indebted to Arabic writers for the earliest undoubted reference to these, a shower of which was witnessed on a date corresponding to Oct. 13 in the year A.D. 902. From that year to the present (1900), 998 years have elapsed or 30 periods of 33·27 years. But Quetelet mentions, in his 'Nouveau Catalogue des principales apparitions d'étoiles filantes' some Chinese records of showers seen many centuries before; and Kirkwood, in his little book on 'Meteoric Astronomy' (p. 123), points out that, although no season of the year is assigned for these, yet that, as the year of the earliest, according to the most probable view of Chinese chronology, corresponds to an exact number of periods before those of authentic occurrences of what are now called the November meteors or Leonids, there is a high degree of probability that the shower noticed was a very ancient one of these. It is stated to have appeared in the fiftieth (the last but one) year of the reign of Kieh Kwei, who is described as being a monster of vice and voluptuousness. It is supposed that he ascended the throne at a date corresponding to B.C. 1818, and he was overthrown and his dynasty supplanted by that called the Shang in B.C. 1766. The fiftieth year, then, of his reign would be B.C. 1768; and from that year to A.D. 902 would be 2669 years, or 80 periods of 33·36 years. Or from B.C. 1768 to A.D. 1900 there are 3667 years, or 110 periods of 33·35 years each.

Yours faithfully,
W. T. LYNN.

Blackheath, 1900, Oct. 11.

The Eclipse of August 17, 1384.

GENTLEMEN,—

It may be of some interest to refer to this eclipse, which appears, by Oppolzer's Canon, to have been just on the limits of totality and annularity, whilst its shadow-path was almost exactly the same as that of the eclipse of last May. The year was that of the death of Wycliffe, and we have a dated record of the eclipse in Walsingham's 'Historia Anglicana,' vol. ii. p. 117, which runs thus:—

"Eodem anno [*i.e.* A.D. 1384] die sexta-decima mensis Augusti, sero et per noctem sequentem, audita sunt in Anglia perhorrenda tonitrus, et visa fulgura terribilis, quæ mortales multipliciter terruerunt. In crastino vero [which would be August 17], post meridiem, quasi per horam facta est eclipsis solis, et conjunctio simul, aspicientibus eas multis."

This is undoubtedly an eclipse in regard to which the *exact* date is given by the historian. According to 'l'Art de vérifier les Dates,' it was central at Paris about an hour after noon; the 'Astronomic Tables' of Dr. Grattan Guinness give $2^h\ 57^m$ Jerusalem time corresponding to $0^h\ 36^m$ Greenwich time) in the afternoon as the true time of conjunction.

Yours faithfully,
W. T. LYNN.

Blackheath, 1900, Sept. 17.

Shower of Aurigids.

GENTLEMEN,—

One of the best defined and frequently visible radiants during the last half of the year is to be found in Auriga a little N.E. of a line connecting the stars ι Aurigæ and β Tauri. I first recognized it in 1876, and have often re-observed it since that year.

It is still visible, for I recorded several meteors from it on Sept. 23 and 27 last, including one very nearly stationary at $75\frac{1}{2}^\circ +32^\circ$ on Sept. 23 at $11^h\ 59^m$. The following 12 radiants have been determined at Bristol:—

	°	°	✶ s.
1884, July 23	75	+31	4
1881, August 27	75	+33	5
1885, September 3–17....	78	+33	6
1879, September 14–21 ..	76	+33	10
1900, September 23–27 ..	76	+32	5
1877, October 8	77	+31	9
1879, October 14–15	76	+33	7
1877, November 7	77	+32	8
1879, November 12–13 ..	77	+33	5
1876, November 16–17 ..	76	+33	5
1885–6, December 24–31..	77	+32	5
1886, December 28	77	+30	S.S.P.

The last position is derived from a 1st magnitude shooting-star observed at Sidmouth and Bristol. The positions of the radiants agree very well. Further observations of the shower are required in the mornings of July and August, and in the evenings from Oct. 15 to Nov. 7, and Nov. 17 to Dec. 24. It seems quite possible from the materials already obtained that radiation from the point $77^\circ +32^\circ$ is continuously maintained during the last six months of the year.

In September the meteors are swift and leave streaks.

As an instance of a shower supplying slow trained meteors, I may mention that this year both on Sept. 1–2 and Sept. 23–27 I saw a fairly active radiant at $333^\circ +57^\circ$. It was well seen at Bristol in Sept. 1886 and 1887, and forms a prominent stream, the various observations of which are tabulated in No. cclvi. of the General Catalogue. But it seems highly probable that two showers are included here, viz.: one at $329^\circ+62^\circ$, and the other at $335^\circ +57^\circ$.

Yours faithfully,
W. F. DENNING.

Bishopston, Bristol,
1900, September 27.

A Correction.

GENTLEMEN,—

Pray allow me to call attention to an error in the footnote on p. 361 of the October number of the *Observatory*, where $d = 2{\cdot}44\ \frac{\lambda}{\Lambda}$ should have been $d=(2{\cdot}44)\ \frac{\lambda f}{\Lambda}$, Λ and f being the diameter and focal length of an object-glass, and d the diameter of the dark ring which bounds the spurious disk of a star when it is formed by light of wave-length λ passing through the whole aperture of this object-glass.

I may take the opportunity of also calling attention to the form of the spurious disk when only part of the objective is used. It is often no longer round. Thus, when the arrangement represented on p. 365 is employed for dividing a double star, the spurious disk of each star is elongated in a direction perpendicular to the line joining the sectors. If desired, this elongation may be prevented by exposing four similar sectors of the object-glass instead of two, the additional ones being at right angles to those shown in the figure. In this case the image of a star becomes a small central spurious disk which is not elongated, with portions of rings surrounding it forming a kind of cross through the spurious disk. The diameter of the spurious disk must exceed $(1{\cdot}414)\lambda f/A$.

For some purposes this type of image may be the best; but in this case there is no longer an absolutely dark line separating the two spurious disks, and probably a sufficiently experienced observer will find the arrangement with two sectors, as shown in the figure, to be the more useful. I am, Gentlemen,

Yours faithfully,
G. Johnstone Stoney.

8 Upper Hornsey Rise, N.,
Oct. 15, 1900.

OBSERVATORIES.

[Continued from p. 382.]

München. *H. Seeliger.*—Observations with the transit were somewhat slackened to permit the reduction of some 16,000 zenith star observations to be pushed forward. The 10½-inch refractor was used by Dr. Villiger for observations of comets and small planets, measures of Saturn's system, and estimations of brightness of Nova Aurigæ, which was between 12·7 and 12·8 mag. in December. Leonids.

O'Gyalla (Ungarn). *N. von Konkoly-Thege.* — Sun-spots. August meteors well observed; weather unfavourable for Leonids. Time service.

Potsdam (Astrophysical). *H. C. Vogel.*—Installation of large photographic refractor, 80 cm. aperture, with a guiding telescope 50 cm. aperture. Drs. Eberhard and Ludendorff took 160 star-spectra with the small spectrograph D. The astrographic refractor (13-inch) was used to obtain 35 star-spectra of long exposure (up to 3^h) for researches on the ultra-violet end of the spectra. The researches undertaken by Dr. Lohse on metallic spectra were continued, and Prof. Wilsing was occupied with his researches on the wave-lengths of spectral lines. Measures of position-angles of snow caps and other parts of the surface of Mars. Diameters of planets. Positions of red spot on Jupiter, and also of other surface markings. Profs. Müller and Kempf continued their work on the photometric zone $+40°$ to $+60°$, and only a small number of stars remain to be done. It would appear from their observations near Milky Way that with decrease of magnitude the percentage of white stars is larger than with the brighter stars. The photometric work on the Pleiades shows discrepancies from the B.D. magnitudes. Under the direction of Dr. Scheiner the work on the Astrographic Chart has made good progress, and the Potsdam zone, +31° to +40°, is nearly complete. The reduction to rectangular co-ordinates is being pushed on. A few double stars were measured with the Schroeder 11¾-inch.

POULKOVA.—The publication from which these notes are extracted contains an ample report of the work done at Poulkova during the last ten years. As no notice of the proceedings at this famous institution has appeared in these pages during that time it is thought expedient to give here for reference, as is done in the 'Vierteljahrsschrift,' * a list of the publications which have emanated from the Russian National Observatory during that period.

1889–91.

Bredikhine ...	On the Perseids observed in Russia in 1890.	B.
	On the Radiant-points of the Andromedes.	B.
Belopolsky......	On the Rotation of Jupiter.	B.
	On the Motion of Sun-spots.	A. N.
	On the Corona Photographs, 1887 Aug. 18–19.	A. N.
	On Surface Movements in the Sun.	A. N.
	Observations of Solar Eruption.	M. S. I.
Nyrén	On Co-operation in observing Latitude Variation.	A. N.
	Observations of Total Solar Eclipse, 1890 June 16–17.	A. N.
Renz	Observations of 51 Double Stars, Wide Pairs.	B.
	Observations of Olbers' Comet, 1887 V.	A. N.
	Observations of Comets 1889 I. and 1889 V.	A. N.
H. Struve	Results of Observations of Saturn's System with the 30-inch Refractor.	A. N.
Sokoloff.........	Pendulum Observations in Poulkova, Warschau, Bobruisk, Moskau, Samara, Orenberg.	

1891–92.

Bredikhine ...	Dispersion of Meteor Radiants.	B.
Belopolsky ...	Spectrum of Nova Aurigæ.	B.
Renz	Observations of Comet 1889 V.	A. N.

1892.

Bredikhine ...	On the Perseids observed in Russia in 1892.	B.
	Orbit of Bielids.	B.
	On the Bielids of 1872, 1885, and 1892.	A. N.
Belopolsky......	Positions of Spots from Solar Photographs at Poulkova in 1881–1888.	Mem.
	Spectrum of β Lyræ.	B.
	Estimation of Solar Rotation from Sun-spots.	A. N.
	Spectroscopic Observations of Motion in Line of Sight of ζ Herculis.	A. N.

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* The list given in the 'Vierteljahrsschrift' has been slightly al-
by omitting memoirs which have for their subject observations not
Poulkova.

Nyrén............	Variation of Latitude as observed with Great Vertical Circle in 1882–91.	B.
	On the Refraction in the Observing Room.	A. N.
Renz	Observations of Comets.	A. N.
Sokoloff	Determination of Motion of Pole from Poulkova Meridian Marks.	B.

1893–94.

Bredikhine ...	Perseids observed in Russia in 1893.	B.
	The Isodynames and Synchrones of Comet 1893 IV.	B.
Belopolsky......	Spectrum of β Lyræ.	M. S. I.
	Reversal of D_3 in the Solar Spectrum.	M. S. I.
Renz	Observations of Nova Aurigæ with the 15-inch Refractor.	A. N.
	Observations of Comets 1892 I. and 1892 III.	A. N.
H. Struve	Observations of Satellite of Neptune with 30-inch Refractor.	Mem.
	Fifth Satellite of Jupiter.	A. N.
	On the Dimensions of Saturn's Disk.	M. N.
Sokoloff	Comparison of the Double Toise N. of the Observatory with the International Mètre.	B.
	Periodic Variation in Motion of Pole.	A. N.
Lindemann ...	Light Curve of β Lyræ.	B.
	Potsdam Photometric Durchmusterung.	V. S.
Ivanoff	Variations of Latitude from Poulkova Observations, 1863–1875.	B.
	Variations of Latitude from Poulkova Observations, 1842–1849.	B.
Ditschenko......	Personal Equation in observing Pole Stars.	B.
Seraphimoff ...	Spots on Jupiter in 1893.	B.
	Observations of Small Planets.	A. N.
Romberg	Results of Zone Observations with Moscow Meridian Circle, 1858–69: Zone 0°–4°.	Mem.
Kostinsky	Parallax of β Cassiopeiæ.	B.
	Variation of Latitude of Poulkova.	A. N.
Kowalsky	Variation of Latitude of Kasan.	

1894–95.

Bredikhine ...	Perseids observed in Russia in 1894.	B.
Belopolsky......	Spectrum of the Variable δ Cephei.	B.
	Spectrographic Researches on Saturn's Rings.	A. N.
	Spectrographic Performance of 30-inch Refractor.	A. J.
Renz	Measures of Photographs of Star Clusters.	B.
	Apparent Places of Stars occulted during Total Lunar Eclipse, 1891 Nov. 15.	A. N.
Lindemann ...	Estimations of Magnitudes of Stars in Cluster *h* Persei.	B.
	Estimations of Magnitude of Z Herculis.	A. N.
Seraphimoff ...	Observations of Small Planets.	A. N.
Ivanoff	Variation of Latitude.	B.
	Observations of Planets with Poulkova V-circle.	B.
	Observations of Mars during Opposition.	B.
	Rotation of the Earth.	

1895–96.

Belopolsky......	Researches on Spectra of Saturn and Rings.	B.
	Spectrographic Researches on Jupiter.	A. N.

	Spectrographic Researches on δ Cephei.	A. N.
	Proper Motion of Bright Component of 61 Cygni.	A. N.
	Changes in Star Cluster N. G. C. 5272.	A. N.
	On the Performances of an Auxiliary Lens for Spectrographic Investigation with 30-inch Refractor.	A. J.
	Observations of Reversals in Prominence Spectra.	M. S. I.
Sokoloff	Observations of Gravity in Paris.	B.
Seraphimoff ...	Observations of Comet 1896 I.	B.
Kostinsky	Personality in Measuring Photographic Plates.	B.
Lindemann ...	Photometric Measures of τ Androm.	A. N.
Nyrén	Referat über Gill's 'Catalogue of 1713 Stars.'	V. A. G.
Wittram	Total Solar Eclipse, 1896 Aug. 8 and 9.	B.

1896–97.

Backlund	Total Solar Eclipse, 1896 Aug. 8.	B.
Belopolsky......	α Geminor. as a Spectroscopic Double.	B.
	Total Solar Eclipse, 1896 Aug. 8.	B.
Hansky	On the Corona.	B.
Ditschenko ...	Mean Positions of 123 Circumpolars.	B.
Ivanoff	Proper Motion of 30 *g* Herculis.	A. N.
Kostinsky	Photographs of Corona, 1896 Aug. 8.	A. N.
Morine	Mean R.A.s of 115 Circumpolars.	B.
Nyrén............	Determination of Aberration Constant.	B.
Seraphimoff ...	Observations of Small Planets.	B.

1897–98.

Backlund	Motion of Small Planets, Hecuba type.	Mem.
	Libration, Saturn's Satellites.	B.
Belopolsky......	Spectra of Variable η Aquilæ.	M. S. I.
	Spectra of β Lyræ.	M. S. I.
	Velocity in Line of Sight of γ Virginis.	B.
	Rapid Motion of Line of Apsides in α Gem.	B.
	Spectrum of λ Tauri.	A. N.
	Velocity in Line of Sight, γ Virginis, γ Leonis.	A. N.
Shilow	Positions of 1041 Stars in Messier 5 from Photographs.	B.
Kostinsky	Position of Exterior Satellite of Mars.	A. N.
	Astronomical Work in Nova Zembla, 1865.	Mem.
Renz	Positions of Jupiter's Satellites from Photographs.	Mem.
	Observations of Satellites of Mars, 30-inch Refractor.	A. N.
Sokoloff	Observations of Comet 1898 I.	A. N.

1898–99

Belopolsky......	Motion of ζ Geminor. in Line of Sight.	A. N.
	Motion of θ Ursæ Maj. in Line of Sight.	A. N.
	Motion of η Pegasi in Line of Sight.	A. N.
Ivanoff	Hülfstafeln for Computations of Orbits of Hecuba type.	B.
	Declination of Standard Stars, Epoch 1845.0.	
Kostinsky	Positions of 413 Stars in Constellation Geminor. which will be Occulted by Moon, 1898, Dec. 27.	

	Photographic Observations of Satellite of Neptune and of Small Planets.	A. N.
	Photography in Stellar Parallax.	Russ.
Orbinsky	Observations of Leonids.	A. N.
Seraphimoff ...	Observations of Small Planets in 1897-8-9.	B.
	Observations of Tuttle's Comet.	A. N.
	On the Andromeda Nebula.	A. N.

References.

A. N. = Astronomische Nachrichten.
B. = Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg.
Mem. = Memoirs of same.
M. S. I. = Memorie della Società degli Spettroscopisti Italiani.
A. J. = Astrophysical Journal.
V. A. G. = Vierteljahrsschrift.

Paris. — The current work of the National Observatory is summed up by M. Loewy in a preface to the Report for the year 1899 presented to the Observatory Council on March 10 last.

The revision of Lalande's zones by meridian observations has nearly come to an end; there remain about 2000 stars to be observed, and when this is done the Gambey instruments will be devoted to determining the places of reference stars for the Astrographic Catalogue. M. Loewy remarks that this revision has become of secondary importance, as the photographic catalogue will supply precise positions of all of Lalande's stars. The Director of the Observatory d'Abbadia, l'Abbé Verschaffel, has offered his co-operation in the Astrographic Catalogue, and has already supplied the place of 3700 reference stars. The reduction of these has brought about a change in the observatory procedure. Since 1854 all meridian observations have been reduced by the help of Le Verrier's catalogue, the star-places of which, corrected from time to time by Paris observations, were taken as fundamental. Now, for the sake of uniformity, the catalogue made by Prof. Newcomb, in accordance with the resolution of the Conference on astronomical constants held in Paris in 1896, will be taken as the clock-star list for all Paris catalogues.

In connection with meridian work, it may be stated that the chronographic registration of transits is shortly to be introduced at Paris, by the use of a chronograph devised by l'Abbe Verschaffel, which, we gather from a brief statement, will print seconds and subdivisions to tenths and hundredths. A chronograph has not hitherto been employed because it was thought that the trouble of reading the transits subsequent to the observations did not outweigh the advantages of the galvanic method. The improvement in the mercury-trough, devised by M. Hamy to eliminate the effect of earth-tremors, is found to work satisfactorily.

In the Astrographic department 30,271 stars on 58 catalogue-plates were measured during the year under the direction of Mdlle. Klumpke. The reduction of the measures, which is effected by another staff, converting them into Right Ascension and Declination, has been continued, and at the date at which the Report closed 23,603 stars on 70 plates were thus reduced. An instrument is being made by M. Gautier for the purpose of determining photographic magnitude.

Preparation of photographs for the exhibition naturally called for some care and trouble on the part of the Director. Some large photographs of the Moon were shown, on the scale of a lunar diameter of $2^{m} \cdot 65$, which required special plates coated with particular emulsion. The help of M. M. Lumiere in preparing these is graciously acknowledged. Examples of the Astrographic Chart were shown, as well as M. Verschaffel's chronograph and M. Hamy's mercury-trough.

M. Loewy refers to the memoir descriptive of the Atlas of the Moon, seven parts of which were published in 1899. He says: "These documents have been studied principally from the point of view of the model of the lunar seas and of the structure of massive mountains. The historic side has equally occupied the attention of the authors. They think they have discerned numerous traces of variation in the level of the seas, and of increase of these in concentric zones. There will be found in the fourth number quite clear instances of convex formations which are thought to form the first outburst of the circles. The distribution of white spots attributed to volcanic ashes has supplied a criterion to assign the relative age for solidification in different parts of the Moon. It also supplies some evidence in favour of the persistence of a feeble atmosphere at the surface of our satellite.

M. Bigourdan has continued with much success his important investigations on the nebulæ. He has, up to now, measured the position of 6000 of these bodies. The same gentleman has also made an investigation relative to earth-tremors resulting from the traffic around the observatory. He finds that vibratory movements are perceived at a depth of 27 metres.

M. Hamy has made a series of measures to determine the wavelength of a certain number of reference lines in the spectrum, and other members of the staff have published researches of various kinds.

MELBOURNE.—The Board of Visitors of this observatory, in reporting to the Governor of the Colony of Victoria on their annual visitation to the observatory, have two matters to refer to in the way of complaint. This is not by any means a complaint as to the conduct of the observatory by the Acting Government Astronomer, Mr. P. Baracchi, for of his work they speak in high terms of praise, but rather as to the way in which he is being treated. When Mr. Ellery retired in 1895, Mr. Baracchi was given the title of Acting Government Astronomer, and as such he has carried on the work of the observatory since that time, but has received no increase of emolument; in fact, the salary he receives is now £400 per year, as it was when he was Third

Assistant, except that he now has an extra allowance of £72 for rent. The second matter of complaint arises thus: there seems to have been a re-classification of offices in the colony, and the post of Government Astronomer has been placed in Class B of the Professional Division of the Public Service; and the Board very naturally urge that this post should be put in Class A. The Re-classification Board certainly seem to have misunderstood or misjudged the type of man who holds such an office and his functions. It should be pointed out to them that the heads of Government observatories are officers of the class who are rewarded for their services with titles.

Mr. Baracchi's report tells us nothing very much out of the usual course, except the progress of the astrographic work, which will be referred to later. The transit-circle has been employed to observe the reference stars for this, and comparison stars for Dr. Gill's heliometer observations: 3311 observations of Right Ascension and 2406 of N.P.D. were made in the year ending 1900, March 31. The great telescope and other equatorials were not used for any systematic work, but only occasionally for examination of comets and planets, and for the amusement of visitors, of whom there were 334 on Wednesday afternoons and 260 at night during the year. The reduction of the magnetic observations of the past 30 years has been continued. A large amount of public work in the way of rating chronometers and testing instruments is done at the Observatory; and now Mr. Baracchi has been asked by the Mining Ventilation Board to undertake the testing of air-meters, and for this purpose he has invented and set up a "whirling machine." A Milne seismograph and a micrometer by Repsold, after Dr. Gill's design, for measuring astrographic plates, have been added to the observatory equipment.

The sections of the Report treating of the measuring of the Astrographic Catalogue plates is signed by Messrs. H. C. Russell and P. Baracchi; for, as was announced last year, the Colonies of New South Wales and Victoria have combined, and each supply part of a sum of money to pay for the photographs taken at Sydney and Melbourne to be measured and reduced. As to the photography itself, the catalogue plates allotted to Melbourne (S. Dec. 65° to 90°) have all been taken with the exception of a few scattered regions, and the zones "above" 80° of declination (which, we infer from another statement, means between 65° and 80° S. Dec.) are being duplicated. The series of chart plates with centres at even degrees of declination, exposure one hour, are all taken, and those with centres at odd degrees, with triple exposures of 30 minutes each, are being taken. The measuring staff consists of six young ladies, who have for their use the Repsold micrometer, which arrived in Melbourne in January last, and another measuring-machine, made in Melbourne, on something of the same plan as the Repsold, but differing from it in that it has only one measuring-screw, and can therefore only measure one co-ordinate without turning the plate or the screw through 90°. With the Repsold 40 to 50 stars can be measured in an hour, with the Melbourne micrometer only 30 or 40. The Report states that this rate could be exceeded if the observers were not to measure the diameters with such care as they do. But Messrs. Russell and Baracchi think that these measures are of sufficient importance to justify the additional expenditure of time.

The greater part of the year was spent in the preliminary work of learning and experimenting. At the date of the Report the positions and magnitudes of 21,000 stars had been measured in direct and reversed positions on 42 plates, and, besides these, 39 plates had been measured for optical and other investigations. A third measuring-machine is being made in Sydney, so that the work may be considered to be in full swing. The total number of stars to be measured is estimated at a million and a half, and it is thought that this may be done in six or seven years.

PUBLICATIONS.

The Royal Observatory, Greenwich *.—The name of the author of this work is well known as that of an astronomer whose speciality lies in that most difficult branch of observation, spectroscopic researches on the motions of the stars. Such work, with its extremely arduous hours of observation, its difficulties, its disappointments, may well have claimed the whole of his time and energies; but he gives ample proof in the book before us of the deep interest he takes in all branches of astronomical research, especially as carried out at the Royal Observatory, Greenwich, the history of which he has culled from every known source and tells again in a delightful style.

About half the book treats of the lives of the Astronomers Royal who have created and gradually brought the Institution to its present magnificent organization. It is not pretended that there is anything new in these chapters, as the lives of such distinguished men of science have been more than once written; but Mr. Maunder skilfully traces the influence of each on the growth of the observatory from being nothing but a small private establishment to its present imposing proportions. Flamsteed and Bradley have had their praises sung before, but besides re-telling the astronomical exploits of these men, Mr. Maunder shows how much astronomy owes to Maskelyne and Pond, whilst the two latest occupants of the office receive due notice. A remark of Airy's anent his predecessor is worth recording:

> For certainty and accuracy, astronomy is quite a different thing from what it was, and this is mainly due to Mr. Pond.

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* 'The Royal Observatory, Greenwich: a glance at its History and Work.' By E. W. Maunder, F.R.A.S. The Religious Tract Society: London, 1900. Price 5s.

The portraits of the eight Astronomers Royal, copied, in most cases, from the same sources as were used for the picture published in this magazine in October 1899, are given in the book.

The second half of the book describes, as popularly as possible, the instruments, the work done in the various departments, the object of and the distressing monotony of the everlasting calculations. The writer of this notice, remembering the years of hard work spent on the reduction of the Transit of Venus observations of 1874, fully agrees with the author when he says:—

> An observer with the transit instrument will take only three or four minutes to make a complete determination of the place of a single star; but that observation will furnish work to the computers for many hours afterwards. Or to take a photograph of the Sun will occupy about five minutes in setting the instrument, whilst the actual exposure will take but the one-thousandth part of a second; but the plate once exposed will have to be developed, fixed, and washed; then measured and the measures reduced, and on this average will provide one person with work for four days before the final results have been printed and published. "It is easy to see, then, that observing, though the first duty of the Observatory, makes the smallest demand on its time."

Mr. Maunder shows how the grand Institution, of which he is himself a distinguished member, has for two centuries not only fulfilled the object of its foundation and endowment—that object being strictly utilitarian—but has also advanced astronomy in more academical branches as occasion has demanded.

The book necessarily treats of subjects difficult, if not impossible, to render strictly popular, which cannot be made in the least degree intelligible to the uneducated; but the author shows great tact in "drawing the line somewhere." As an excellent example of description one might note the account on p. 197 of the every night operation of observing a star by reflexion with the transit-circle and mercury-trough, where the action is so vividly described as to make the reader almost as breathless as the observer himself. The reason of the operation is stated in a few words, which will doubtless be understood even by the least astronomical reader.

We began the chapter on the Meteorological Department with a sinking of the heart; but were soon reassured, as here, as in every other department, Mr. Maunder shows himself quite at home in the subject, which he succeeds in an eminent degree in rendering both interesting and instructive. Scarcely a word escapes him—where it would have been pardonable—about the part he has himself taken in maintaining the past reputation of the Institution.

The book has evidently been very carefully revised, as we have not detected a single error among the figures or careless statement of fact. A slip of the pen occurs (p. 234), where the temperature of evaporation appears to be confounded with the temperature of the dew-point.

The volume is copiously illustrated with pictures of buildings and instruments of the Observatory, chiefly from photographs by Mr. Lacy and others, members of the staff, and we lay this charming book down with the intention of reading it again before long. G. L. Tupman.

Publications of the Lick Observatory, Vol. IV.—This volume contains the first series of meridian observations made at Lick by Mr. Tucker. The observations extend from 1893 Sept. to 1896 June, and have been somewhat delayed in printing owing to causes beyond the control of the observatory. The list, comprising 310 standard stars, and including all stars north of −40° declination, which are given in the American 'Ephemeris,' the British 'Nautical Almanac,' and the 'Connaissance des Temps,' but are not included in the 'Berliner Jahrbuch,' was designed to supplement the comparison made by Auwers, published in the 'Berliner Jahrbuch,' 1884, which gives the corrections necessary to reduce any star found in that ephemeris and in one of the other ephemerides to the B. J. system. The observations have been reduced with a list of 86 equatorial stars from B. J. as fundamental for both right ascensions and declinations, and are therefore entirely differential; but corrections are given depending on observations of the nadir (three of which were made each night), and a standard latitude derived from the circumpolar observations on the list. The observations were made with a Repsold reversible transit-circle, virtually the duplicate of the one at Strasburg. The objective and ocular are interchangeable, so that the observations depend on four different positions of the instrument. The transits were recorded on a chronograph. The observations have been evidently made with the greatest care and attention to details, and the results show a remarkably small range. Poulkova refractions were used in the reductions of the declinations. The telescope was not clamped, the resistance of the collar to which the clamp is attached being sufficient to insure against movement from any slight pressure. No corrections for division errors have been applied to the observations, and the value of the flexure, which is small, is derived from observations on the horizontal collimators.

The observed values of latitude from the B. J. stars are as follows:—

32 polar stars (both culminations) ……	25″·52
86 equatorial stars …………………	25 ·65
22 stars at zenith …………………	25 ·77

The differences here shown, Mr. Tucker says, may be explained by a bisection error obtained from an independent source.

The probable error of one observation by comparison of separate determinations with the mean of four is:—

R.A.	$\pm 0^{s}{\cdot}019$ sec δ up to 70° Dec.
	$\pm 0\ {\cdot}016$ „ for circumpolars.
Dec.	$\pm 0''{\cdot}20$ at the equator.
	$\pm 0\ {\cdot}42$ at Z.D. 77°.

The effect of graduation error is not included in this comparison. The following are the systematic differences for the fundamental stars, and the observed corrections to the ephemerides for stars not in B. J., from which each system appears consistent with its zone of equatorial stars, and also the extension of the B. J. system at the equator to the limits of this series by these observations:—

Fundamental Stars.			Stars not in B. J.		
B. J. − A. E....	$-0^{s}{\cdot}026$	$+0''{\cdot}05$	Obs. − A. E....	$-0^{s}{\cdot}028$	$+0''{\cdot}02$
B. J. − C. T....	+0·015	−0 ·22	Obs. − C. T....	+0·016	−0 ·24
B. J. − N. A....	+0·021	+0 ·18	Obs. − N. A....	+0·008	+0 ·02
			Obs. − N. A. '97	+0·025	−0 ·71 (−30°)
B. J. − 303 ...	0·00	−0 ·51	Obs. − 303 ...	+0·013	−0 ·75 (−14°)

The following is a comparison, arranged in order of declination, between the Lick series of observations and Prof. Newcomb's standard star-places, given by Mr. Tucker as an Appendix, the corrections to the A. G. system, printed in *A. N.* 3508, 3509, affording material for comparing Prof. Auwers' new standard places with those of Prof. Newcomb:—

No. of stars.	Lick. Dec.	Lick − Newcomb, 1895·0.	
	°	s	"
18	85·7	−0·10 sec δ	−0·07
39	74·8	−0·27	−0·02
8	68·5	−0·33	−0·11
1	54·4	−0·09	−0·33
5	41·0	−0·33	−0·18
30	36·5	−0·24	−0·15
11	23·4	−0·37	−0·09
25	13·6	−0·39	−0·20
54	+ 5·3	−0·23	−0·18
47	− 5·3	−0·23	−0·38
21	16·1	−0·06	−0·52
22	26·2	−0·01	−0·51
28	35·4	−0·13	−0·86

Of course it is impossible to say how closely the Lick observations thus corrected actually represent Auwers' new fundamental places, for they are not yet published. W. G. T.

NOTES.

Minor Planet Notes.—The following is a continuation of the ephemeris of Eros (for Berlin midnight):—

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Nov. 1....	2	15	8	53	51	Nov. 21....	1	36	54	53	5
5....	2	7	10	54	14	25....	1	31	49	52	10
9....	1	58	56	54	21	29....	1	28	23	51	2
13....	1	50	51	54	12	Dec. 3....	1	26	43	49	44
17....	1	43	22	53	47						

The planet is now of about magnitude 9½, and its brightness steadily increases throughout November, as it is approaching both the Sun and the Earth.

Six new planets have been discovered by Wolf at Heidelberg.

The first five were all on the same plate, viz., FM, FN, FO, FP, FQ, discovered on October 22. This is, we believe, the largest haul yet made with one cast of the photographic net.

The sixth planet, FR, discovered on October 23, may not improbably prove to be identical with (177) Irma. A. C. D. C.

Double Stars.—In *A. N.* 3668 Mr. Aitken publishes a second list of 62 double stars found mainly with the 12-inch refractor at Lick. This brings his total up to 109. Twelve of these latter stars are under 0″·5 apart.

Prof. Campbell says that one of the components of κ Pegasi is a spectroscopic double. During the time of his observations the two components (visual) were never separated by more than 0″·3, consequently it is difficult to decide which is the spectroscopic double.

The Opposition of Eros.—The work for determination of the parallax of Eros by co-operative effort is now in full swing. The last circular issued by the Committee gives the names of 21 more observatories who have given their adhesion, which makes up more than 50 in all. As was stated in our September number, the original scheme demanded that a series of observations, photographic or otherwise, should be made in large hour-angles, east and west of the meridian, with a supplementary series about two hours after the first series. The Committee now suggest that as, at this season of the year, bad weather may cause many gaps in the observations, several supplementary series should be made—in fact that observations should be made whenever possible. They point out the desirability of observers taking especial care to determine their screw-values and instrumental constants generally.

At Greenwich the Astrographic Equatorial and the Thompson

Refractor are both being employed to take photographs, in the early evening and before sunrise, with supplementary series as suggested; but the weather at present has not allowed the programme to be carried out in entirety on many nights.

Fireballs.—On Sunday evening, October 21, several brilliant meteors were visible, and have been described in the newspapers by observers in various parts of the country. At $8^h\ 35^m$ there was a magnificent slow-moving meteor, which is referred to by the *Daily Mail* as a "huge" object and one of the brightest seen in an English sky for many a year. It was seen at London, Stafford, Bristol, Bridport, Llanelly, Tewkesbury, Cardiff, Billingsborough (Lincolnshire), and many other places. From a preliminary discussion of the observations, Mr. Denning finds the radiant at $308° - 15°$ in Capricornus, and the length 37 miles over a point 10 miles W.N.W. of Hereford to 25 miles over Bridgnorth, Shropshire. Path 37 miles; velocity 10 miles per second. This probably represents only a part, and the best observed portion, of the meteor's flight. At $9^h\ 30^m$ on the same evening another large meteor appeared, but this does not seem to have been so well observed as that at $8^h\ 35^m$.

The November Meteors.—It seems almost bold to say anything in the way of prediction of the appearance of the Leonid shower, remembering the disappointments of last year and the year before; but our readers need not be reminded of Mr. Denning's series of articles in this magazine for the year 1897, where, in giving the probable times of appearance of the shower in successive years, he stated that in this current year 1900 we shall be immersed in the thickest part of the stream just before sunrise on November 15, when the Moon will be near last quarter, so that if the meteors do strike the Earth then, the display may be brilliant. In saying this it is quite realized that the observations, or rather lack of observations, last year point to a perturbation of the meteor orbit, which may have caused the main stream to pass clear or nearly clear of the Earth's path, but our knowledge of the present condition of things is so vague that hope of seeing a shower need not be given up. The wise observer will keep an eye on the sky from midnight to sunrise during the mornings of November 14, 15, and 16.

Relation between Gravity and Temperature.—A note in *Ciel et Terre* states that Col. von Sterneck, at the request of the Academy of Sciences of Vienna, has been making researches to determine whether there is any variation of g with temperature, as has been suggested. The experiments, which consisted in measuring the attraction of gravity at the bottom of mines of various depths, appear so far not to indicate any conclusive result, it being difficult to eliminate the effects of local temperature. As one result of the experiments, Col. Sterneck determined the density of the Earth, 5·52.

THE UNIVERSAL TIME-SYSTEM.—The American *Science* is responsible for the announcement, doubtless correct, that Spain has adopted Greenwich Time. The exact wording of the decree runs:

> In all railway, mail (including telegraph), telephone, and steamship service in the Peninsula and Balearic Islands, and in all the ministerial offices, the courts, and all public works, time shall be regulated by the time of the Greenwich Observatory, commonly known as Western European Time.

Also the hours of the day are to be numbered from 1 to 24. The hour of midnight will be called 24, but the time between this and 1 o'clock will be designated as $0^h\ 5^m$, $0^h\ 10^m$, $0^h\ 59^m$.

THE WEATHER IN OCTOBER.—A generally fine month, with a remarkably small rainfall of small amounts distributed over many days. Until the 24th, 0·49 inch was measured on nine days, but during the latter part of the month more copious falls on five days after the 25th brought the month's rainfall up to 1·55 inch, being 1·30 inch below the average for the preceding 59 years. Temperature, as is usual in an autumnal month, fluctuated considerably, two hot days on October 8 and 9, on which the thermometer rose to 73°·0 and 74°·2 respectively, being followed within a week by a great fall of temperature, the minimum for the month, 35°·6, occurring on the 16th, and the range in the week amounting to 38°·6. In the 59 preceding Octobers there have been 9 instances of higher temperature than that recorded this month, the highest October temperature on record being 81°·0, which occurred on October 4, 1850. The sunshine in the month amounted to 110 hours, being about 25 hours in excess of the average for the preceding ten years.

THE RADCLIFFE OBSERVATORY.—From paragraphs in the daily press we learn that the building designed by Mr. T. G. Jackson, R.A., to house the new telescope at the Radcliffe Observatory is nearly finished. The following description is from the *Daily News*:—

> The building is circular, and is 35 feet in external diameter; it is provided on the lower floor with a large door, solid rounded steps, and two full-sized windows of the ordinary square sash type. The walls rise to a height of 34 feet; at about two-thirds of this height a bold cornice of stone and red brick is introduced, and above this eight elliptic windows, or "eyes," are placed, with bold dressing of rubbed red brick. These form the chief ornamental feature of the tower, and are surmounted by a wide hard stone cornice, upon which is bedded the iron sole-plate of the great hollow dome, which will rise around and above the telescope.
>
> Within the building, and almost central, is built the massive brick
> which the enormous cast-iron base of the telescope will stand. The

rests on a solid mass of concrete, which is placed at a depth of 17 feet below the surface of the ground. The weight of this concrete block is estimated at about 30 tons. The great hollow dome which surmounts the tower is nearly spherical in shape, and its ribs are of steel. These are at the present time being covered with papier maché, which forms the protecting roof. The whole dome revolves easily on wheels united by a "live ring," and rises to a height of 53 feet above the ground.

Sir Howard Grubb is making the rising floor as well as the telescope.

The Royal Observatory of Belgium.—It is announced that M. Charles Lagrange has resigned the position of Astronomical Director of the Uccle Observatory, to which he was appointed in 1896, in succession to M. Folie, and his resignation has been accepted. It appears that he asked to be relieved of his office two years ago, and though the Government, loth to lose an officer of such value, asked him to reconsider his decision, M. Lagrange had reasons for his action which compelled him to adhere to his request. M. Georges Lecointe, Lieutenant of Artillery, has been appointed to succeed him.

The American 'Nautical Almanac.'—It is announced in the American Journal *Science* that Prof. H. T. Todd, having reached the age-limit, has retired from the Directorship of the U.S. 'Nautical Almanac,' and that Prof. S. J. Brown, Astronomical Director of the U.S. Naval Observatory, has undertaken the duties of the office. We regret to think that we have not any announcement in our pages of Prof. Todd's appointment to the office, of which, indeed, we had not before heard; but in any case his tenure of the post must have been of short duration, for the preface to the N.A. for 1902, which is dated 1899 June, is signed by Prof. Harkness. The age-limit as applied in public offices appears to have disadvantages in some cases. In accordance with this regulation Prof. Newcomb retired from this office in April 1897. Prof. Brown appears to be the third occupant of the post since that time, whilst it is evident from facts given in a note below, that Prof. Newcomb is still in possession of his youthful energy and faculties.

We have received from Messrs. Stoneham copies of new editions of two little books by our valued contributor, Mr. W. T. Lynn. The title of the first, 'Astronomy for the Young,' explains the matter of the book; it only remains to say that the manner is excellent, and the author's name is a sufficient guarantee for its accuracy. Our readers need not be told that Mr. Lynn is an authority on chronology, so that they will not be surprised that the title of his other work is 'The Penny Chronology.' It gives the dates of the principal events of history from Abraham to the Boer war, and is full value for two halfpennies.

THE following paragraph is from the *Literary World*:—

> Messrs. Constable will shortly publish a work on Astronomy by Mr. H. H. Turner, F.R.S., Savilian Professor of Astronomy at Oxford. It will be a record of the changes which have taken place in the methods and scope of astronomical instruments during the last quarter of a century. Another book on the same subject, but of a more elementary nature, will be a primer by Sir Robert Ball in the Cambridge Science Series.

THERE is a funny little story in Mr. Burckhalter's account of the Chabot Observatory Eclipse Expedition to Siloam, Georgia, in the Publications of the Ast. Soc. Pac. for October. Prof. Burckhalter was one day making sextant observations for position, when his coloured assistant, who was something of a character, being interested, asked what he was doing. The Professor told him he was trying to find exactly where he was. "Foh de Lawd's sake! why didn't you ask me," said the nigger; "I was done bo'n and brought up heah! You's *right heah in Siloam.* I reckon you's foolin' me!"

THE University of Cambridge has, *honoris causa*, granted Prof. S. P. Langley, Secretary of the Smithsonian Institution, and Director of the Astrophysical Observatory at Washington, the degree of Doctor of Science. Prof. Langley's inventions, the bolometer and the aerodrome, were thus referred to by the Public Orator :—

> Luminis in spectro, ut aiunt, infra radios rubros radii alii qui oculorum aciem prorsus effugiunt, viri hujusce ingenio, instrumenti novi auxilio quod *βολόμετρον* nominavit, paulatim proditi et patefacti sunt. Nemo mirabitur virum stellarum observandarum amore tanto affectum, etiam e terra volandi desiderio ingenti esse commotum,—adeo ut quasi alis novis adhibitis, plus quam trium milium pedum per spatium, etiam avium volatum æmulari poterit. Fortasse aliquando, Icari sortem non veritus, etiam Horatii præsagia illa sibi ipsi vindicabit :—
>
> "Non usitata nec tenui ferar
> Penna, biformis per liquidum æthera."
>
> Fortasse rerum terrestrium impatiens, rerum cœlestium avidus, ausus erit e terris
>
> "volare
> Sideris in numerum, atque alto succedere cœlo."

FATHER HAGEN, the Director of the Georgetown Observatory (he will be remembered as the compiler of an excellent atlas of variable stars), is now in England. He visited Greenwich recently in company with Father Strassmaier, whose personal acquaintance we were glad to make. M. Vallé, the new Director of the Tacubaya Observatory, is also in this country. We believe that Sir David Gill will shortly return to his official duties.

As many of our readers probably do not see the American publication called the *Popular Science Monthly*, it may be well to mention that Prof. Newcomb is writing a very excellent series of articles therein, called "Chapters on the Stars." The September number contained chapters on "Star Spectra" and the "Motion of the Sun in Space"; the chapter in the October number was "Variable Stars"; earlier chapters in the series were on the names and the number of the stars—each being a masterly treatise, but not too scientific on the subject. We have reason to know that Prof. Newcomb contemplates publishing a book shortly; perhaps these are to form part of it.

Prof. W. W. Campbell has been appointed Acting Director of the Lick Observatory until a new Director is appointed and assumes the duties of his office.

Prof. Michelson, of the University of Chicago, has been awarded a Grand Prize at the Paris Exhibition for his Echelon Spectroscope.

A preliminary discussion of the diameters of Neptune and Uranus is given by Dr. See in *A. N.* 3663.

There will be an annular eclipse of the Sun on November 21, visible in South Africa and West Australia.

The next Meeting of the Royal Astronomical Society will take place on November 9; of the British Astronomical Association on Wednesday, November 28.

From an Oxford Note-Book.

Quietly, almost insidiously, the Opposition of Eros has crept upon us. There has been no preliminary excitement such as preceded the Transits of Venus; no practising of observers, and fitting out of expeditions to all quarters of the globe. There was a little gathering of astronomers in Paris after the Astrographic Conference to talk over matters; a few circulars (of the greatest value) have been sent out from Paris; and, with no further rehearsal, here we are in the thick of the observations, taking photographs night and morning whenever we can, and expecting to do so for some four or five months. But it is probable that no harm has been done by the absence of more definite preparations. The real preparation is the work which has already been done in dealing with the Astrographic Catalogue, and such special details as are necessary for this work on Eros were very quickly learnt from the first few photographs. Even the preliminary talk at Paris was more or less in the air; for some of the anticipated difficulties, notably that arising from the rapid motion of Eros, have not troubled us at all in actual work. To be sanguine is always risky, but I fancy we are embarking on a successful enterprise, and though it has not brought actually together those working in concert, as sometimes happens with astronomical enterprises, I look round metaphorically to my colleagues and submit the sentiment "To the good parallax!"

PART of this hopefulness is due to the simplicity of which the present operations are found to be capable; and part is due to Sir David Gill. Within the last few days I have been carefully through those huge volumes which discuss the observations of Victoria, Iris, and Sappho; not, of course, through every figure of them, but through every argument and difficulty, and have emerged with two definite impressions: the first an admiration for the zeal, care, and ability of all concerned in these great works; and the second a conviction that our difficulties, whatever they may turn out to be (and we shall no doubt find quite plenty), will not be of the kind which have been so ably dealt with in these volumes, viz., those arising from personality in the observer. To get rid of this puzzling and uncertain difficulty is surely a valuable gain, and gives us a right to start on our venture hopefully: time enough to worry over unforeseen difficulties when they arise. On the other hand, perfect honesty requires confession of a haunting apprehension lest we find a parallax differing from 8″·80, and I trust Sir David Gill will appreciate the compliment!

To dilute the severity of these volumes I have been "sailing alone around the world" with Captain Joshua Slocum. What a delightful trip and what an achievement! To lash the helm for twenty-three days on end (covering 2700 miles) and sit below in the library reading a book! Well may a man who has done this say that "the stories of sea danger are for the most part exaggerations," especially when he makes it clear in other parts of the book that he knows what sea danger is. And when danger came he was "prepared for every emergency" with "a kit of not too elaborate carpenter's tools, a tin clock, and some carpet tacks"! The tools, one may well believe, got their value from the hand that used them: the tacks were for the naked feet of unwelcome intruders on the decks: but the tin clock! I wish it could be acquired for Greenwich Observatory, to be stored alongside Harrison's chronometer. Captain Slocum had a chronometer before he started, but as it would have cost fifteen dollars to repair he left it at home. His tin clock cost a cheap dollar, "on account of the face being smashed." When he had been some time at sea it lost its minute-hand, but "after I boiled her she told the hours, and that was near enough on a long stretch"; so that when Dr. Gill visited the *Spray* and Captain Slocum visited the Cape Observatory, some interest was manifested by each in the clock of the other. "He [Dr. Gill] showed me the great astronomical clock of the observatory, and I showed him the tin clock of the *Spray*, and we went over the subject of standard time at sea, and how it was found from the deck of the little sloop without the aid of a clock of any kind." But it must not be supposed that Captain Slocum did not know what accuracy meant; he took what must have been some excellent lunar distances, and when they did not agree with his own ideas of where he was he fearlessly suspected *and found* an error in the table of logarithms. "The tables being corrected, I sailed on with self-reliance unshaken, and with my tin clock fast asleep." Wonderful man! And he does not forget to add a tribute of admiration for the astronomer's work in making the tables which give such information. "The work of the lunarian, though seldom practised in these days of chronometers, is beautifully edifying, and there is nothing in the realm of navigation that lifts one's heart up more in adoration." But I could go on quoting for a long time; the fact is, everyone should get the book* for themselves, and at least every observatory library should possess it. I can, however, scarcely resist one more little extract, about the man who drew a large comet on the side of his shop and hung out the sign:—"This wicked world will be destroyed by a comet! The owner of this store is therefore bound to sell out at any price and avoid the catastrophe."

THE above-mentioned misprint in the table of logarithms reminds me of another little story. There is a certain Dr. Falb who makes predictions of weather, and on one occasion the weather so pointedly refused to follow the course laid down for it that it was necessary to find some excuse. Accordingly we read in one of the daily papers next morning that "Herr Falb had made an error in his logarithm." And doubtless Herr Falb accordingly, like Captain Slocum, "sailed on with self-reliance unshaken," though the shake to the confidence of others would be different in the two cases.

TALKING of weather-prophets, I have been expecting for nearly two years to hear of some sensation consequent upon the arrival of one who left England for the States in January 1899. He tried for some time to get an audience in England, but at last obtained some advice. "I am advised," he wrote on January 5, "to try to gain attention to my meteorological views in the United States." I wonder who gave that rather selfish advice? and whether he would be touched by reading the following rather pathetic sentence from the same letter? "If I should have the honour of making known some of the fundamental laws of meteorology at present not understood, there is no doubt my fellow-countrymen will be benefitted, and they will be able to say, in good part, that I left my country for my country's good." Poor man! What a life of continual disappointment the paradoxer leads! And yet it does not do to be gentle with them; at least it only prolongs the business, with the same inevitable termination.

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* 'Sailing Alone Around the World.' By Captain Joshua Slocum. Sampson Low, 1900.

Concerning the new *Liste Générale* of Astronomers and Observatories, M. Lancaster asks me to say that he is specially desirous of having the geographical co-ordinates of any observatories which have sprung into existence in the last ten years, or whose positions have been redetermined. Also the titles of new publications, especially those with a small circulation. And finally information as to changes in the *personnel* of observatories would, of course, be welcome. With the view of troubling his correspondents as little as possible, M. Lancaster remarks that a *post-card* to him (M. A. Lancaster, Directeur du Service météorologique de Belgique, Uccle, Belgium) is all that is necessary; and as the work is for the general benefit, he hopes for the same cordial co-operation as on former occasions—a hope which will be generally shared.

The following Roundel to a "non-smoking Astronomer" is taken from a delightful little book called 'La Santa Yerba, written by William L. Shoemaker,'* sent me by an American friend. The illustrious initials G. W. H. will readily be recognized. There are several of these pretty little poems in praise of "the weed" which I should like to quote, but I fear they have no astronomical excuse.

Nicotian Roundels.

I.—*Urania and Euterpe.*

To a non-smoking Astronomer: G. W. H.

Excuse me, Hill—that you don't smoke I know,
But, really, I my pipe must frequent fill;
And if in air a copious cloud I blow,
Excuse me, Hill.

Your Muse, that soars to farthest planet, will
No smoke about her path ethereal throw—
Urania's nose tobacco scent brooks ill;
But mine †, who loves this smoky orb below,
My pipe disdains not, helps me to distil
Some poesy from its fragrant fume, and so
Excuse me, Hill.

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* Boston, U.S. Copeland and Day, 69 Cornhill: 1898.

† Euterpe, or Euterpia, from the sweetness of her singing. Some call her Tibicina, because, according to them, she presides over pipes. — *Tooke's Pantheon.*

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1900 November 9.

Mr. E. B. Knobel, *President*, in the Chair.

Secretaries: H. F. Newall, M.A., and F. W. Dyson, M.A.

The Minutes of the previous Meeting were read and confirmed.

Mr. Dyson. One hundred and eighty-eight presents have been received since the last Meeting. Among the books there will be found:—Volume ii. of the late Prof. J. C. Adams's Scientific Papers, presented by the Adams Memorial Committee; Part ii. of Prof. André's 'Traité d'Astronomie Stellaire,' presented by the Author; Groningen Astronomical Laboratory Publications, Nos. 1–3, by Prof. Kapteyn and Mr. W. de Sitter; a volume of the Astronomische Gesellschaft Catalogue, Leipzig Zone I.; American Nautical Almanac Papers, vol. v. parts 1, 2, and vol. viii. part 1; also a copy of Mr. Maunder's book on the Royal Observatory, Greenwich, presented by the publishers.

Among photographs and drawings are 70 enlargements on paper of Astrographic Chart plates, taken at the Observatories of Paris, Algiers, San Fernando, and Toulouse; Fascicule 4 of the 'Atlas Photographique de la Lune,' from the Paris Observatory; a collection of astronomical drawings made by our late Fellow, Mr. N. E. Green, presented by Miss Green; a portrait of the late Prof. Keeler, from the Lick Observatory; Prof. D. P. Todd sends a photograph of the Transit of Venus, 1882, and also a picture of instruments used for eclipse observations. We have also received presents relating to the late eclipse from Mr. Newbegin, Mr. C. Burckhalter, and Prof. Young, of Princeton. Dr. Downing has kindly sent a publication of the Batavia Natural History Society, giving information useful to observing parties for the eclipse of 1901 May.

We have also had presented to us a small equatorially-mounted telescope, formerly belonging to Mr. B. T. Moore.

A vote of thanks was accorded to the donors.

Mr. Crommelin (presenting a paper on the eclipse diameter of the Moon). Experience has shown, both in 1898 and 1900, that the computed totality in solar eclipses is too long. It seems to me probable that this is caused by the great irregularities in the Moon's limb. The value of the semi-diameter used in the *Nautical Almanac* for eclipses and occultations, 15′ 32″·65, was deduced by Dr. Ludwig Struve from numerous occultations during lunar eclipses. This therefore would be the average semi-diameter, taking as a standard the level intermediate between the highest mountains and the deepest valleys; but since the Sun is not totally eclipsed so long as any portion is visible through a lunar valley, the really effective semi-diameter for producing totality should only be reckoned to the level of the deepest valley. This might well make it some 0″·9 less, which would agree with the value 15′ 31″·76, used by the American Ephemeris. I suggest that the *Nautical Almanac* should use this or even a slightly smaller value for the prediction of totality. It is curious that in the American Ephemeris for 1902 the value of the semi-diameter used for eclipses and occultations is 15′ 32″·55. The adoption of this value for eclipses seems to me to be a retrograde course.

Mr. Seabroke. I am not sure that I heard the figure correctly; did Mr. Crommelin say 0″·9?

Mr. Crommelin. 0″·9 is the difference between the radii.

Mr. Seabroke. About what depth would that correspond to?

Mr. Crommelin. 5000 feet.

Mr. Evershed. I should like to call attention to the fact that at my station at Pont Mazafran the mountains on the limb, I believe, were about 35,000 feet in altitude, and the Sun was shining through a very deep valley. It must have been an extremely deep valley in these mountains; whether it was beyond the occultation value of the radius I cannot say. It might be well to try and identify that spot.

Mr. Dyson. That shows there are points where there are deep valleys, but Mr. Crommelin implies there are deep valleys all round the limb of the Moon, and they surely would be visible.

Mr. Franklin-Adams. Those who have seen Prof. Barnard's photographs of the prominences and corona will agree with Mr. Crommelin's remarks. Those who have not seen them will do well to study them in connection with this paper.

Mr. Chambers. I should like to evoke some expression from observers as to Baily Beads. It seems to me it is a very vexed question. I was very much struck when I saw, both before the eclipse and afterwards, at Portugal that very uniformity of shape and size which has been referred to. It seems to me, having regard to the fact that the lunar mountains are so exceedingly irregular, that there was wonderful symmetry in shape and size.

Mr. Maunder. I do not quite sympathize with Mr. Chambers's view. As they appeared to me in this last eclipse I did not see anything about the phenomenon to cause me to call into question the usual explanation that they are due to irregularities on the Moon's surface.

Rev. C. D. P. Davies. I may mention that I have seen one irregularity on the Moon's limb under extraordinary circumstances, and that was during the occultation of Saturn. I saw it as it fell across the planet most distinctly.

Capt. Noble. Of course there are isolated mountains. I have myself noticed occultations of stars which disappeared and reappeared, and then again went out. It must have been a mountain of large size to occult a star and then allow it to be seen again. It seems to me that the edge of the Moon cannot be as the edge of a circle.

A Fellow. But it must be remembered that we are not looking on a surface, but on a sphere, and the mountain would interfere with the valley.

The President. In regard to Mr. Chambers's remarks, which are well worthy of consideration, as to Baily's beads not being explained by the existence of lunar valleys, it must be remembered that it needs a vast number of observations to settle a point of that description.

Mr. Wesley then exhibited on the screen several photographs by Prof. Burckhalter, and said that these photographs seemed to be of very great promise because of the amount of detail that was shown. To his thinking the appearance that was given by these pictures, and those seen by the eye were strikingly similar, although in the latter case there was not so much detail.

Dr. Spitta. Can you tell us how he worked the shutter?

Mr. Wesley then described on the blackboard how the shutter was worked.

The President. There is a very remarkable result gained by these mechanical contrivances which will be of great advantage to those who will observe later eclipses, and it seems to me to be a most valuable contribution to the Society.

Mr. Newall then read a paper by *Dr. Isaac Roberts* on the disappearance of star-images from photographic films, and their recovery by a chemical process. Dr. Roberts had stated that by a chemical process it was possible to secure the reappearance of stars that had disappeared from the photographic film, but this statement was received with some doubt by Sir William Crookes, who said that he would be willing to try an experiment if Dr. Roberts would send him a negative, which Dr. Roberts gladly did. The negatives were sent, and in a short time they were returned to Dr. Roberts, and it was found that the images had been restored to view as distinctly as they were after their negatives were first developed, showing, therefore, that the experiment had succeeded admirably. Dr. Roberts gave in the paper the formula and the process adopted by Sir Wm. Crookes. It practically came to this, Mr. Newall said, that the plates were re-developed, re-cleared, re-washed, and finally drained on blotting-paper and allowed to dry. It was a most important method of dealing with a difficulty which Dr. Roberts had pointed out. The fading of a negative was a serious thing, because it frequently happened that in an observatory negatives were collected more quickly than they could be dealt with. He himself had plates that were twelve months old; but fortunately he had been greatly favoured so far. The statement by Sir Wm. Crookes that washing the plates very well was necessary, was confirmatory of the statement made by M. Lumiere last summer in Paris.

The President. This point is one of very great interest, but the secret of the process for restoring the image here is merely the process of toning, which is the final step usually taken in restoring an image that is faint to view.

Mr. Maw then exhibited a series of slides, accompanied by notes, of Prof. D. P. Todd's apparatus for observing total eclipses.

The President. It is a little difficult to follow the application of some of the parts, pictures of which have been thrown upon the screen; but we know Prof. Todd's experience in eclipse work, and we are indebted to Mr. Maw for having given us this description this evening.

Mr. Newall then read a paper on the variable velocity of α Persei. The spectrum of this star was photographed with a four-prism spectroscope, giving very large dispersion, and the plates were measured with an instrument in which one revolution of the micrometer corresponds to 40 kilometres per second. The paper gives a table summarizing the results of the measurements made by myself and my assistant, Mr. A. W. Goatcher. There seems to be a systematic difference of the order of about 2 kilometres per second between our results, but the difference does not affect the broad facts established as a whole. The observations, which extend over a period of 50 days, suggest a period of 4·2 days, and I show a diagram with the observations all thrown in the usual way into one period. You will see that they fall on the descending branch of the curve with extraordinary accuracy. This immediately arouses suspicion. For when, as in the present case, the range of velocity is small, viz. from +8 to −4, there are many pitfalls in choosing a short period. This is well exemplified by the fact that I find at the last moment that a period of about 16·8 days will fit the observations if the orbit is of considerable eccentricity; but the material at my disposal is not sufficient to decide between the two alternatives suggested. At Potsdam in 1888 the velocity observed was 10 kilometres per second, and Professor Campbell gives results of four determinations in 1896–1898, viz., −2·0, −1·8, −3·5, −2·1. More material is required, and it should be gathered in a short period; but at Cambridge in November we have not an atmosphere in which one can readily get photographs of α Persei, when the light has to pass through 22 inches of glass.

The President. Has Prof. Vogel made any other observations of the star?

Mr. Newall. Not that I know of. It happens that the two Potsdam observations are taken in an interval of less than 5 days.

Mr. Thackeray. May I ask what is the probable error of one of these determinations?

Mr. Newall. Each one of the determinations I have given is derived from about eleven lines in the spectrum, and I am not able to state, without referring to my note-book, what the numerical value of the probable error is, but I came away with the idea that I might trust my determinations to 2 kilometres per second. I may say that I recall a case in which a plate was measured twice by the same observer, and the velocities deduced were 2·7 and 3·0 from the same plate; but as to the statement of probable error, I cannot say anything definite without reference to my note-book *.

Prof. Turner. I remember when Dr. Gill came back from Potsdam after seeing the photographic measures of velocity in the line of sight, he was so enthusiastic about Prof. Vogel's measures that he said we should soon be determining the solar parallax from such determinations. At that time I was sceptical, but one is rather inclined to that belief now when one sees such results as these. I think we should be prepared to admit the possibility that the solar parallax might be determined in that way were it not for one fact, and that is that no star will stand still long enough to enable the Earth's orbit velocity to be measured with reference to it.

The President. The Meeting this evening is honoured by the presence of M. Vallé, the Director of the Observatory of Tacubaya, Mexico. I will ask him to say a few words to the Society.

M. Vallé described to the Society in detail the instrumental equipment and work that is being done at his observatory.

The President. I am sure I shall be expressing your feelings in according M. Vallé our hearty thanks for his interesting address. We hope he will be successful in securing the data for the Astrographic Chart work which he is now undertaking, as he has succeeded in the other work he has told us of.

Mr. Bellamy showed photographs taken at the Oxford University Observatory of the planet Eros as it passed a small star. Each plate was exposed to the sky five times, a small displacement being given to the telescope between the exposures. The developed plate therefore showed five images of the star in a line, and the planet appeared in a different position relative to each of these five images. Though the planet approached very close it did not occult the star at any time.

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* The only satisfactory way of arriving at a probable error in these cases would be to deduce it from many photographs of a star whose velocity is constant. The only case I can at present give is that of α Canis Minoris; three separate determinations of the velocity give the results −3·0, −2·9, −3·3. —H. F. N.

Figures illustrating the accuracy of the measures of Eros were shown on the screen.

The Astronomer Royal. I have been very interested to see the results obtained at Oxford. It might be of interest to show one or two plates which have been taken at Greenwich. Of course, we have also been working upon this subject of Eros. I have not drawn up any formal note about the observations which have been made because we are in the middle of the work, but photographs of Eros have been taken at every opportunity at Greenwich from July 18th and onwards with the Thompson 26-inch equatorial and with the Astrographic equatorial. We have obtained photographs east of the meridian, on the meridian, and west of the meridian on eighteen nights, viz.:—47 with the Thompson equatorial and 52 with the Astrographic. [Lantern slides made from some of these were shown.] I think there will not be any difficulty from the motion of Eros during the short time which is required for the exposure. Besides the four exposures of 3^m and 4^m given as shown here, a fifth supplementary exposure of 1^m has been given for orientation, and we find the results satisfactory. The photographs taken with the Thompson are on double the scale, the chief advantage of which is in the lines of the réseau. The conclusion that we have come to is that there is not much gained by the increased aperture of the Thompson in this work, that the chief error will not come in the measurement of the lines of the réseau, but in measuring the image of the planet: and we have not found that the image, when magnified to the same scale, is appreciably better with the Thompson 26-inch refractor than with the 13-inch, and practically the same exposure is required in the two cases.

Prof. Turner. Allow me to say a word. After the very frank admission of the Astronomer Royal it is a very great comfort to know that we who have not got a 26-inch can do as good work with a 13-inch.

The President. The latest novelty in astronomical observation is the application of the biograph to solar eclipses, and we are fortunate in having with us this evening the presence of Mr. Nevil Maskelyne, who will be kind enough to show us a biographical result of the solar eclipse of May last. I invite Mr. Maskelyne to favour us with some explanation of his apparatus, and to show us the results.

Mr. Maskelyne. The great fault which will be evident in the photograph is due to the unsteadiness of the cinematograph, and this was due to my not being able to get the instrument sufficiently rigid at the time the photographs were being taken. During totality I obtained 300 pictures, each of which had about one-fifth of a second exposure, so that the interval of time from the beginning of one and the beginning of the next exposure was so infinitely small that the light was hardly cut off at all.

Mr. Maskelyne then showed the results obtained.

At the request of Prof. Turner the pictures were shown again, with the strip of film reversed, so that the end of the eclipse was seen first.

The President. I am sure I can congratulate Prof. Maskelyne on the singularly ingenious and interesting exhibition he has put before us. It is the first time we have seen anything of the sort, and we are much interested. We shall look forward to the time when Mr. Maskelyne has perfected his instrument, and we may see an eclipse of the Sun without all the expense and annoyance of making such journeys as we have to at present. We thank him for the pleasure he has given us this evening.

The President. The next business we have to do is to ballot for new Fellows. I desire to remind Fellows that there is an obligation on them to take part in this ballot. It is their duty to do what they can to ensure the election of candidates whose worth will enrich the ranks of our Society. At the same time they should exercise some discrimination, so that no undesirable candidate should be elected. The Bye-Law states that no election shall take place without there shall be twelve Fellows present. That evidently contemplates that at least twelve Fellows would take part in the ballot. I have had nearly twelve years' experience on this platform, and I am sorry to say I have never seen this duty neglected so seriously as it was at the last Meeting. It should never be in the power of a single Fellow to secure the rejection of a candidate, and I feel it is my duty to make these remarks and to invite Fellows to take part in this election of candidates.

The following papers were announced and partly read:—

H. A. Howe. "Observations of Nebulæ made at the Chamberlin Observatory, Colorado."

E. M. Antoniadi. "On the Appearance of Saturn's Crape Ring in 1900."

J. Gledhill. "Observations of Jupiter and his Satellites at Mr. Crossley's Observatory, 1899–1900."

F. P. Leavenworth. "Photographic Measures of the Ring Nebula in Lyra."

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon for 1901."

A. C. D. Crommelin. "Note on the Moon's Eclipse Diameter."

Rev. S. J. Johnson. "Occultation of Saturn, 1900 September 3."

Isaac Roberts. "On the Disappearance from Photographic Films of Star Images, and their Recovery by a Chemical Process."

Rev. C. D. P. Davies. "Note on the Total Eclipse of the Sun, 1900 May 28, observed at Algiers."

Royal Observatory, Greenwich. "Micrometric Measures of the Diameter of Neptune and Satellite made with the 28-inch Refractor."

F. A. Bellamy. "Photographic Observations of Eros: a close Approach to a small Star."

H. F. Newall. "On the Variable Velocity of α Persei."

J. L. E. Dreyer. "Corrections to the Armagh Catalogue for 1840."

Radcliffe Observatory, Oxford. "Occultation of Saturn, 1900 June 13 and September 3."

Royal Observatory, Greenwich. "Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1899, deduced from Photographs taken at Greenwich, at Dehra Dûn (India), and in Mauritius."

The following gentlemen were elected Fellows of the Society:—

A. H. Baker, B.A., 28 Cautley Avenue, Clapham Common, S.W.

W. H. Colegrave, Little Tew, Enstone, Oxford.

Comte Meredyth de Miremont, Littlecot, Worthing, Sussex.

The following Candidates were proposed for election as Fellows of the Society:—

Chas. Anthony, Jun., M.Inst.C.E., Casilla 1045, Buenos Aires (proposed by Sir R. S. Ball).

H. A. Barnard, Supt. Trigonometrical Survey, Ceylon (proposed by Capt. Molesworth).

A. Y. G. Campbell, I.C.S., Assistant Collector and Magistrate, Board of Revenue, Madras (proposed by Sir R. S. Ball).

Chas. Davidson, Royal Observatory, Greenwich, 41 Park Street, Greenwich, S.E. (proposed by W. H. M. Christie).

F. C. Dumat, Advocate, Johannesburg, S. Africa (proposed by E. Nevill).

A. T. Flagg, M.A., Navigation School, Chapel House, Westoe, South Shields (proposed by Thomas Lewis).

Walter Heath, M.A., Redcott, Cobham, Surrey (proposed by J. W. L. Glaisher).

J. C. W. Herschel, B.A., St. John's College, Cambridge, and Lawn Upton, Littlemore, Oxford (proposed by Sir R. S. Ball).

Capt. J. W. Martyr, The Glen, South Road, Forest Hill, S.E. (proposed by Capt. C. Hepworth).

A. E. Moore, B.A., B.Sc., Lecturer in Mathematics and Physical

Science, St. John's College, Battersea, S.W. (proposed by A. Fowler).

J. N. Jutsum, Cardiff Nautical Academy, 47 St. Mary Street, Cardiff (proposed by Arthur Mee).

T. M. Nightingale, B.Sc., 375 Bridgman Street, Bolton (proposed by A. Fowler).

Richard Welford, Thornfield Villa, Gosforth, near Newcastle-on-Tyne (proposed by C. Hutchinson).

THE BRITISH ASTRONOMICAL ASSOCIATION.

THE tenth Annual General Meeting of the British Astronomical Association was held on Wednesday, October 31, at Sion College, Victoria Embankment, *Mr. W. H. Maw* (the President) in the Chair.

The names of nineteen candidates for membership were read and passed for suspension, and the election by the Council of nine new Members was confirmed.

The scrutineers of the ballot (Messrs. E. Holmes, J. J. Vezey, and L. B. Tappenden) reported the election of the following Officers and Members of the Council for the ensuing session:—*President*: Mr. G. M. Seabroke. *Vice-Presidents*: Capt. Noble, Dr. Isaac Roberts, Mrs. Walter Maunder, and Mr. W. Schooling. *Treasurer*: Mr. W. H. Maw. *Secretaries*: Messrs. J. G. Petrie and A. C. Crommelin. *Librarian*: Mr. F. W. Levander. *Other Members of the Council*: Dr. Downing, Mr. J. E. Gore, Mr. A. Kennedy, Mr. F. McClean, Mr. G. J. Newbegin, Miss M. A. Orr, Mr. S. A. Saunder, Mr. D. Smart, Mr. C. Thwaites, and Mr. C. T. Whitmell.

The election by the Council of the following Directors of the Observing Sections was next confirmed: *Sun*, Rev. A. L. Cortie; *Moon*, Mr. W. Goodacre; *Mercury and Venus*, Mr. H. McEwen; *Mars*, Mr. E. M. Antoniadi; *Jupiter*, Mr. A. Cottam; *Saturn*, Mr. G. M. Seabroke; *Comets*, Mr. A. C. D. Crommelin; *Meteors and Auroræ*, Mr. W. Besley; *Star-colours*, Mr. E. W. Maunder; *Variable Stars*, Col. E. E. Markwick; *Double Stars*, Mr. G. M. Seabroke; *Spectroscopy*, Mr. J. Evershed; *Photography*, Mr. R. Wilding; and *Zodiacal Light*, Capt. P. B. Molesworth.

A vote of thanks was passed to the retiring Members of the Council, Miss Bacon, the Rev. J. M. Bacon, and Messrs. Chambers, Denning, Rambaut, Johnstone Stoney, and Wesley.

Mr. Maunder wished to add one word, and he felt sure that he would be speaking the mind of the entire Association, in which to express his great regret at the failure of health which had compelled Mr. Denning to retire from the direction of the Meteoric Section. Not only had Mr. Denning been of great service to the Association in filling that post, but he had conferred a great honour upon them by doing so. He was sure that all would feel the utmost sympathy with Mr. Denning for the cause which had obliged him to resign the directorship of the section, dealing with that field of work in which he had accomplished such great achievements and won such well-deserved laurels.

Mr. W. H. Maw, the outgoing President, then delivered the annual address. He first referred with regret to the death of Prof. Keeler, and after reviewing the position of the Association, numerically and financially, he proceeded to speak of the recent solar eclipse, of the reasons which led to the abandonment of the expedition on a large scale under the auspices of the Association, and then to the work which had actually been done by Members. He spoke at some length on Prof. D. P. Todd's eclipse instruments. In conclusion Mr. Maw said:—If, leaving eclipse matters, we look through the records of astronomical work during the past year, we shall find no sensational discovery; but we shall find what is far better—namely, evidence of a vast amount of valuable work diligently and carefully performed. In every department of astronomical research there has been progress, and progress of the kind that promises further advances in the future. No one, I think, who carefully follows the modern developments of any particular line of inquiry can fail to be struck with the increased attention to detail and the extreme care to avoid, or to determine the disturbing effect of, small sources of error which characterize recent work. Some of this is undoubtedly due to the improved training in physical work which is afforded by many of our technical colleges; but it is also due to the general recognition amongst men of science of the importance of what has been termed "the infinitely little." We see this in all branches of science—in chemistry, in metallurgy, in physiology, in the study of constructional materials, in spectroscopy, and certainly in astronomy. The day has long passed when a scientific man worthy of the name would not think it beneath him to build up elaborate theories on data which, from their lack of the necessary accuracy, we should now regard as utterly inadequate to support any theory at all. It is true that from time to time sensational theories relating to astronomical matters are still advanced and are put before the unwary public in specious forms which cause them to be too often accepted by those who are unable to criticise them. But such theories, I am glad to say, now rarely emanate from serious workers, and the keynote of modern astronomical research is undoubtedly a desire to take nothing for granted, and an earnest striving after accuracy, which promises the best results for the future. A natural result of this state of things is that steady progress is being made in the improvement of the numerous instruments and appliances which the modern worker calls to his aid. Not only are instruments of standard types much better designed and constructed than formerly, but new instruments specially adapted for certain definite lines of research are continually being devised and constructed—mostly under rigorous conditions which the older instrument makers would have found it practically impossible to meet. As a rule, too, these special instruments are originated by those using them, for it is only such users who have a full knowledge of the necessary requirements. It would be of interest, if time permitted, to trace the manner in which the progress in some particular line of inquiry has gradually necessitated the introduction of new methods of working and new appliances for carrying on such work; but this is impossible on the present occasion, and I can only call attention to the general fact that success in modern astro-physical research work is, as a rule, as largely due to skill in devising appliances for carrying out such work as it is to care and skill in the use of such appliances when constructed. The gist of these somewhat desultory remarks is that it appears to me that astronomy offers to the mathematician, the mechanician, and to the worker in almost every branch of physical science, a practically endless series of problems awaiting solution. If the science of astronomy is to progress, as it promises to do, that progress must be the result of work of the most varied kind, carried out by an army of workers with widely different qualifications and predilections. In other words, astronomy is presenting year by year wider and wider interests, and as this fact becomes understood, we have a right to expect that this growth may be accompanied by a corresponding growth in both the importance and usefulness of the Association whose success we all have at heart.

Dr. Downing, in proposing a vote of thanks to the President, not only for his admirable address, but also for the services he had rendered the Association during the two years of his presidency, which were just terminating, remarked that Mr. Maw seemed to think it desirable that the custom of the President delivering an address at the annual meeting should be abandoned, but he (the speaker) hoped it would be maintained. Not only was the preparation of the address an admirable discipline for the President himself—and he (Dr. Downing) had filled that office himself, so he ought to know—but it was also admirable from the point of view of educating those to whom it was addressed. In further remarks, Dr. Downing bore testimony to the courtesy and ability with which Mr. Maw had presided over the meetings of the Association, and also over the deliberations of the Council during the past two years.

Mr. Maunder seconded the motion, which, after some remarks by *Mr. Avenell* and *Mr. Seabroke* was carried unanimously.

Dr. Downing spoke on the subject of the Leonid meteors. He said that he had been engaged, with Dr. Johnstone Stoney, in computing the perturbations to which the Leonid swarm had been subject during the last thirty-three years, and they had come to the conclusion that the part of the swarm with which the Earth collided in 1867 passed last November about 1,250,000 miles within the Earth's orbit, and that this year the distance of the meteors was probably about 1,750,000 miles, so that the only chance of seeing a display was in the event of the stream being sufficiently broad to reach to the Earth. The most likely time for them to be seen would be about 3 A.M., and watch should be kept on the nights of November 13th, 14th, and 15th.

Under the title of "Astronomy with the South African Field Force," the *Rev. J. T. Bird*, Chaplain to H.M. Forces, recorded some interesting astronomical observations which he had made while in South Africa. Perhaps the most striking of these was his observation of the Zodiacal Light, and, concurrently, the rising of the Moon (then only about 40^h from "new") illumined by Earth-shine. The distinctness of the Zodiacal Light and the brightness of the Earth-shine on the dark body of the Moon were both to be attributed to the same cause—viz., the extreme clearness and transparency of the atmosphere.

Mr. Seabroke said the remark made by Mr. Bird as to seeing the dark body of the Moon between himself and the Zodiacal Light rather tended to dispose of certain theories as to the origin of that light. The observation showed that the light had its origin beyond the Moon, at any rate. As the writer said, the light could scarcely have been due to the light from the Moon in the atmosphere, because in that case the dark body of the Moon would not have been seen.

Mr. Maunder, The President, Mr. Crommelin, Mr. Thwaites, and *Mr. Holmes* joined in the discussion. The last-named gentleman said he did not think much reliance could be placed on what occurred in Tropical latitudes, after what he read recently in a magazine that professed to insert only true stories. There was a tale of a lady and gentleman who got benighted in the bush in a Tropical latitude, and they decided to wait until the full Moon arose at 11 o'clock to find their way out. (Laughter.)

Mr. E. Antoniadi contributed a further paper entitled "Considerations on the Double Canals of Mars," on which *Mr. Maunder, Mr. Goodacre,* and *Mr. Holmes* spoke. *The President*, in illustration of the fact that persons sometimes unconsciously built up an image of what they expected to see, said if the greatest distance at which print of a certain size could be read was found, and then print of that size, but in a foreign language, was put up at that distance, one unacquainted with that language would have the greatest difficulty in deciphering the letters.

Mr. Crommelin gave an abstract of a paper by *Mr. W. Godden* under the title of "The Actinic and the Visible Corona."

The Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

The opening Meeting of this Society for the present Session was held on Wednesday evening, November 21, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. C. Theodore Williams*, President, in the Chair.

A communication was read from the International Meteorological Committee, inviting observations of the form, amount, and direction of the clouds on the first Thursday of each month during 1901, as well as on the preceding and following days. These observations are to be made in connection with the balloon ascents which will be carried out under the direction of the Aerostation Committee.

Mr. R. H. Curtis exhibited an improved mounting for the lens and bowl of the Campbell-Stokes sunshine recorder, by means of which the glass ball can be quickly and accurately placed centrally in the bowl, where it is secured by clamping screws.

Mr. W. H. Dines read a brief paper on the "Weekly Death-Rate and Temperature Curves, 1890–99." He exhibited diagrams showing the death-rate of the 33 great towns of England, and also curves of the temperature at Greenwich. The author is of opinion that from the health point of view the English climate is one of the best in the world; and this is proved by the relative low rate shown in these curves. A pleasanter climate may well be found, but the majority of health-resorts to which Englishmen resort in the winter have a higher death-rate than London has at the same season, and a far higher rate than any of the country districts of the British Isles.

The Zodiacal Light.

[Continued from p. 398.]

It was reserved for Cassini to direct attention to its examina'ion for the first time with earnest inquiry and interest. His first notice of it was on the evening of the 18th of March, 1683. He was watching in the west for other things, but was struck with the appearance of this luminous streak reaching far up in the sky. Like most discoverers, Cassini immediately formed a theory in regard to it after making but ten observations. This hypothesis, based on very insufficient data, has continued to the present time to influence the opinions of astronomers, and has retarded interest in observing its phenomena. Like most theories of the heavenly bodies in their first origin, it was doubtless erroneous, and, like the Ptolemaic theory of the solar system, it has materially interfered with the establishment of a correct knowledge of what is the truth.

Cassini discovered very soon that as time advanced through March and April, the upper or northern edge of this light inclined more and more from the ecliptic, and stretched farther to the northward, and knowing that the Sun's equator, as shown by his spots, was also then stretching off from the ecliptic in a similar way, he came to the conclusion that the substance giving this light was closely connected with the Sun's equator, and was consequently changing its position with regard to that equator.

He argued, further, that as the Sun has an atmosphere, and is therefore capable of emitting dense vapours, and is continually sending out matter of exceeding fineness which we call light, consequently this luminary might also, by its motion on its axis, send out a substance intermediate in character between the two; which substance, either self-luminous or by reflection, might give us the zodiacal light. To support this theory he gives to this body a lenticular shape, about twice the thickness of the Sun as seen in March, but only of the Sun's thickness when seen in June. Whether he meant to have this lenticular-shaped medium to be attached to the Sun, and revolving with it at the same time, is not apparent. He devoted a part of his time for about eleven years, in a very desultory manner, to observing this light.

Cassini's labours led other observers to direct their attention to the zodiacal light. Fabio de Duillier, who was his colleague for a while at Paris, is worthy of particular notice, as having conceived the idea that it consisted of particles of matter distinct from the Sun, and arranged in shape like a solid zodiac; which body of uneven surfaces, and rotating round the Sun, he supposed, gave us the zodiacal light.

In 1731 Mairan gave considerable attention to this subject in a work on 'The Aurora Borealis.' He advanced the theory that this light is reflected from the Sun's atmosphere, stretched out into a flattened spheroid or lenticular-shaped body, revolving with the Sun—an idea which Laplace has for ever set at rest by demonstrating that the Sun's atmosphere can extend no further than to the orbit of a planet whose periodical revolution is performed in the same time as the Sun's rotary motion about its axis, or in twenty-five days and a half; that is, only so far as nine-twentieths of Mercury's distance from the Sun.

Since the time of Mairan, until 1853, but little attention appears to have been given to this subject. In 1833, however, Biot, in order to account for the meteor-shower of that year, attempted to show that the shower was owing to the Earth's passing at that time near the node of the zodiacal light. But calculations were made by J. C. Housseau in order to see whether the nodes of the Sun and the zodiacal light do actually correspond. After careful observations and calculations, in which he was assisted by nine diligent observers, Housseau thinks he has shown that these nodes are different, and that therefore "the supposition of the existence of this light in the plane of the Sun's equator does not satisfy the observations made." The closing sentence of his very interesting article gives the first intimation that the zodiacal light has a near connection with our globe. Housseau says :—

> One is struck, without doubt, with the near approach, which our elements show, between the line of nodes of the zodiacal light and that of the nodes of the terrestrial equator upon the ecliptic. This circumstance, as far as it is verified, may help to explain the causes of this luminous phenomenon—causes which are, it may be, more local than has hitherto been supposed.

This notice is the only one, of any importance whatever, that was taken of the zodiacal light for many years, with the exception of some experiments made to ascertain whether there is any heat connected with it — experiments which we think resulted in establishing the fact that it is near enough to emit heat sufficient to affect a thermo-electric pile of twenty-five pairs, causing the needle of the galvanometer to indicate 12° when the pile was directed toward the base of the zodiacal light.

We come now to a consideration of the observations taken during the cruise of the Japan Expedition, and we hope our readers are not weary of the subject; for, if it can be demonstrated that we have a ring of nebulous matter around our globe at a distance of 150,000 to 200,000 miles, there are many inquiries growing out of that fact full of interest, and yet to be answered by experiment and induction. It will be a matter, then, to be considered whether it is self-luminous—that is, of the nature of the Sun's photosphere—or whether it shines only by reflected light, like the rings of Saturn; whether it is increasing or diminishing in magnitude and extent. If it is shown to be self-luminous, we will then wish to know whether it may not be widening and extending itself, at the same time increasing in its capacity to emit both light and heat, until it envelops the entire globe, modifies climate, melts the icebergs and snows of northern latitudes, and dispels darkness from our globe. We may enquire, too, whether it may not be used as a grand instrument in the hands of the Creator for the future development of his purposes in regard to our globe. It may also be an enquiry whether it does not already influence the magnetic state of the Earth, and play an important part in the causes producing the aurora borealis. These and many more similar questions will arise from the establishment of the fact that the zodiacal light emanates from a ring of nebulous matter so near our Earth. Let us, then, give our author a close examination and a patient hearing, that we may be enabled to form a correct judgment as to the truth of his deductions.

We have not space to present all the reasoning by which our author shows that the zodiacal light cannot be produced by a ring of nebulous matter revolving round the Sun, either within or beyond the orbit of the Earth.

He starts with the proposition that such a ring must be either —1. Within the Earth's orbit; 2. Surrounding the Earth; or, 3. Without the orbit of the Earth.

It cannot be *within* the Earth's orbit; if so, we could never have it at any great altitude at any period of the night, and we could never have the zodiacal light at midnight on both horizons simultaneously.

We omit the diagram constructed for the consideration of the theory which supposes this ring to be around the Sun and *beyond the orbit of the Earth*, by which he shows that this supposition will not account for the lateral changes observed in the zodiacal light. He says :—

> The query arises, "Can such lateral changes, so uniformly observed, as the evening or morning advances, agree at all with the idea of a nebulous ring giving us this light at a distance from the spectator of 160,000,000 or 180,000,000 miles?" A ring of the character supposed, it seems to me, could give us a zodiacal light only of one uniform shape, namely, with the opposite borders receding from each other for a short distance from the apex, and then running parallel, one to the other, the whole way down to the base. Nor could the hourly changes of time produce any other changes in these boundaries than a rising or sinking of the apex of the light; the boundaries, say at nine o'clock P.M., extending a little higher in the sky than at eight, and so continued, with a parallelism of the opposite sides, down to the horizon. How different this from the true facts of the case!
>
> The evident and most decided connection between these boundary-lines and the spectator's place, as regards the ecliptic, is also a matter of the greatest significance in drawing our conclusions in regard to the origin of this light. Now, supposing the base of the zodiacal light to be at a distance of 200,000,000 miles, how is it possible that the fact that the spectator is a short distance north or south of the ecliptic can govern the reflection from the nebulous ring at that immense distance, and place it on his side of the ecliptic? If he is on the north side of the ecliptic, not only is the main body of the light down to its base on that side, but the lateral changes of the boundaries, as the hours pass, are altogether or chiefly on that side; and so equally with the south. If he is on the ecliptic or near it, the zodiacal light stretches equally, or nearly so, on each side of that line. Also, if he changes rapidly during the night to or from the ecliptic (as was the case on shipboard) the boundaries of this light also change, being regulated by his motion (from one place to another). That the apex of the zodiacal light, from such a ring around the Sun, might be so affected by the spectator's position is not unreasonable, but that the boundary-lines should be so affected seems to be utterly inadmissible.
>
> It is worthy of remark, also, how even and uniform, from apex to base, the change in the boundary-lines is as the hours change; as if the substance giving the zodiacal light were not only near, but also at one uniform distance from the spectator; the portions of it at the apex and base of the light all equally affected by his changes on the Earth.

He shows by Bouguer's law of reflected light that the zodiacal light, if from a ring of nebulous matter beyond the Earth's orbit, would emit a light of the same intensity the whole way from the base to the apex. But another law of optics, that the strength of light is inversely as the squares of the distances of the object affording the light, would here make its application; and this ring, at our zenith, being by that supposition about 140,000,000 miles nearer to us than at the base, we should then have the zodiacal light far more intense at the apex than at the base; all of which is entirely opposite to the facts of the case.

He offers, now, as a last conclusion, *the hypothesis of a nebulous ring with the Earth for a centre.* He makes certain deductions from the examination of the preceding theories.

They are—1. That the substance giving us the zodiacal light must be *equally near* to us in all its parts, inasmuch as the lateral changes of the light, *i. e.*, the changes of boundaries, have a uniform character and mostly a parallelism in their whole extent from apex to base; 2. That no part of this substance can be very remote from us, inasmuch as the outlines of the light were clearly and sensibly affected by my own position on the globe, and even by my change of position in a single night; and, 3. That the laws of reflected light require an arrangement or a shape of this nebulous matter which will give us, at the base of the zodiacal light, larger angles between the lines of the incident and reflected light than at other portions, and also a regular decrease of such angles from the base to the apex of the light, as produced by such a shape. These three requirements appear to be fully met by an hypothesis which, if the theories examined are untenable, is now the only one remaining to us.

The hypothesis is that the zodiacal light is *a ring around the Earth.*

The thought is a somewhat startling one, yet startling only from its novelty; for it is entirely in accordance with what we know of our sister planet (Saturn), and also with the whole of Laplace's celebrated theory of the formation of globes.

We will not take up time and space with quoting his application of that theory in explaining some of the phenomena of this light.

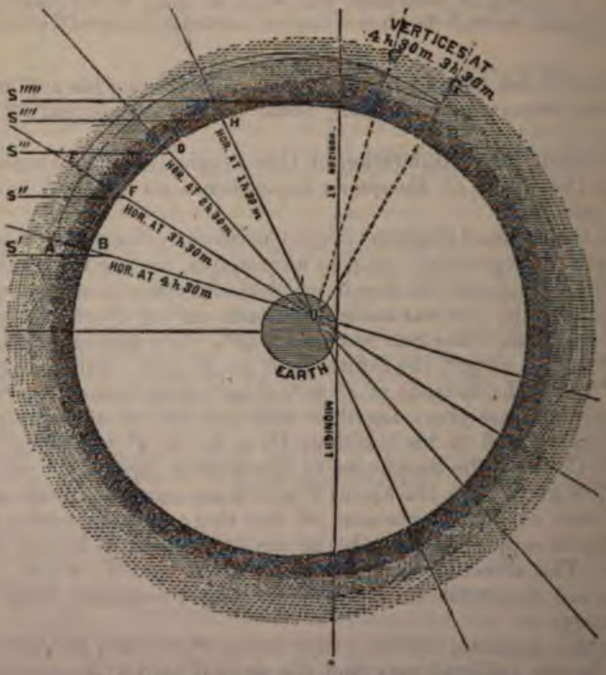
Avoiding the consideration of these topics, we will proceed to apply the result of Bouguer's experiments on reflected light to this case.

In the annexed diagram (p. 446) he takes an observation made on the 4th of September, 1854, as an example, for the reason that it is a simple one, and one also in which the spectator is near the plane of the ecliptic. It was made in latitude 22° 18′ North, longitude 114° 10′ East. The Sun rose at $5^h\ 48^m$. The stronger light was at $3^h\ 30^m$ to $4^h\ 30^m$, the diffuse light at $3^h\ 45^m$. Sun's longitude 161° 35′. The horizons at $4^h\ 30^m$, $3^h\ 30^m$, $2^h\ 30^m$, and $1^h\ 30^m$, and at midnight, are given, together with the line of the spectator's vertices, as well as his positions O, o, &c., at $4^h\ 30^m$ and $3^h\ 30^m$. A, B, C, F are the boundaries of the zodiacal light at $4^h\ 30^m$, and E, F, G at $3^h\ 30^m$; the apices C and G are carried a little above the more condensed portions of the ring; but the reader is at liberty to suppose them to be at any other part, as he may think best. The direction of the Sun is given, and S′, S″, S‴, S⁗, S′′′′′, are supposed to be rays of light proceeding from that luminary.

In this diagram, the Sun's rays being S′, S″, &c., BO, FO, &c., will be the reflected rays, and the several angles between these lines of incidence and reflection, together with the number of rays reflected to the eye, out of 1000 incident rays, according to Bouguer, are in the following table:—

Angle.		Rays reflected from Smooth Water.	Rays reflected from Plate-glass not quicksilvered.
S′, BO	161°......	343	422
S″, FO	146°......	184	270
S‴, DO	131°......	101	162
S⁗, HO	116°......	59	105
S′′′′′, at midnight...	90°......	18	25
S′′′′′′, CO	67°......	18	25

We find in the above table a strong argument for such a ring around the Earth. The figures, taking either of the two columns, for water or for glass, correspond in a very striking degree with the varying intensity of the zodiacal



Nebulous Ring, with the Earth for its Centre.

[The relative proportions of the Earth and the ring, and also its distance, are of course not given in this diagram with any effort at certainty; the upward extent of the ring is probably far greater than can be here represented. The diagram is, however, sufficiently correct for our present purposes of elucidation.]

light from the base upward, as we have it on any clear morning or evening when the ecliptic is at a high angle with the horizon, and when, consequently, the nebulous figure is not brought angularly to our eye. They also correspond to what is, indeed, almost synonymous with what has been stated—namely, to the fact that at $4^h\ 30^m$ the zodiacal light at the horizon is far greater at its base than it is at $3^h\ 30^m$; at $3^h\ 30^m$ than it is at $2^h\ 30^m$, &c., back to midnight. Any person who has ever looked attentively at this light when making a high angle with the horizon will see at once the coincidence between the proportions of the figures in the above table, showing the number of reflected rays, and what has been always presented to the eye. If the reader will also carry these lines of incident and reflected light beyond the midnight horizon-line, to any point there of the nebulous ring, he will see how we may easily get what is referred to in my charts under the German name of *Gegenschein*, *i. e.* dim light seen, when the circumstances are favourable for it, in those portions of the sky opposed to the Sun. This hypothesis shows also very clearly how I could have the zodiacal light above both horizons at the midnight hours, as I was often able to do, and it harmonizes fully with the strength of the light as then presented to the eye. While there are some things still left unexplained, I have yet not been able to see anything in this hypothesis antagonistic to the facts of the zodiacal light. On the contrary, almost all of them are explained by it; and they all, as I can perceive, fully harmonize with it through the whole of the manifold change which the light underwent, either from the changes of the ecliptic toward any fixed point, or from my numerous and great changes of latitude during our cruise.

If we could have a zodiacal light of an undoubted character produced by the *full Moon*, not only would the question before us be set at rest, but the ring would be shown to be within the orbit of the Moon, and how near we came to a case of the kind on the evening of February 14, 1854, the reader will decide for himself. . . .

This ring must, according to the laws of matter, rotate on its centre; and it must be full of commotion within itself. The existence of *pulsations* seems scarcely to admit of a doubt, recorded as they have been by observers in such distant quarters of the globe.

In conclusion, we would say that two simultaneous observations, made in equatorial regions—for instance, one by an observer at Quito, and another by an observer on the island of Sumatra, in which both observations presented the zodiacal light stretching as an arch across the heavens from east to west—would, it seems to us, demonstrate the fact that it is a ring around the Earth. Now, if observations taken in almost every space of 15° of longitude have been made and its existence demonstrated, does not that amount to about the same thing?

We leave the subject, inviting information and discussion from all who are informed in regard to this long-neglected phenomenon.

On the Form of Planetary Ephemerides.

The form in which an ephemeris of a planet is computed is one which has been handed down from a time when the transit-circle was the only instrument of precision, and the convenience of that instrument appears alone to be consulted in the form of ephemeris generally adopted.

Another form of ephemeris is here suggested, suitable for what may be called Astrographic methods of observation. The principle of this method is that the position of the planet referred to the mean equator and equinox of some epoch (say 1900·0) is inferred from its position on a photographic plate relatively to stars whose places are supposed known. The question of precession and nutation does not enter into the reduction; the differential effect of aberration and refraction is entirely eliminated as far as the first powers of the co-ordinates on the plate, and the terms depending on the second order are seldom of importance, and the result is that with comparatively little trouble the mean place of the planet at the time the light left it, as seen from the observer's position on the Earth at the time the light reached it, is obtained.

A small point that does not properly come into the scope of this paper must here be mentioned. Aberration is often applied to planets by antedating the observation, thus allowing for the whole of the planet's velocity and that of the Earth's as well. Now the astrographic method only corrects the planet for aberration in the same way that the stars have been previously corrected; thus it does not allow for the planet's motion. But further than this, it does not allow for the whole of the Earth's motion, for stellar aberration corrects only for the variable part of the Earth's velocity over and above its average velocity, which is about one-sixtieth part of the whole (thus giving rise to an aberration of one-third of a second), and directed parallel to the minor axis of the orbit towards a point in the ecliptic whose longitude is 190°. The method of applying stellar aberration corrects to the centre of the Earth's circular hodograph and not to its origin.

When the position of the planet has been obtained from the plate, and when small corrections have been applied on the above account,

$$\Delta a = -\frac{1}{15} 0''{\cdot}33 \sec \delta \sin (a - 10^\circ),$$

$$\Delta \delta = -0''{\cdot}33 \sin \delta \cos (a - 10^\circ) + 0''{\cdot}02 \cos \delta,$$

where a, δ are the R.A. and declination, it remains to compare the observed place with the ephemeris. Now, as ephemerides are now constructed, this entails the following processes:—(i) The ephemeris, if corrected for planetary aberration, must have the part of the correction depending on the Earth's motion removed; if uncorrected for aberration, it must be corrected for the planet's motion: (ii) The correction for precession and nutation which has been laboriously, and for our purpose unnecessarily, inserted, must be as laboriously removed.

I wish here to suggest that ephemerides should publish the computed values x, y, z of the planet's heliocentric co-ordinates.

X, Y, Z being the simultaneous values of the geocentric co-ordinates of the Sun, the "Transit-Circle Ephemeris" may be completed in the usual way, taking $x+X$, $y+Y$, $z+Z$ as the geocentric co-ordinates of the planet, reducing to R.A. and N.P.D. and applying precession and nutation, and post-dating the time if desired. The "Astrographic Ephemeris" may be completed as follows:—Let X′, Y′, Z′ be the geocentric co-ordinates of the Sun at the "light time" interval subsequent to the instant when the heliocentric co-ordinates of the planet were x, y, z (and it may be observed that this light-time is only wanted with a slight degree of accuracy that can be easily obtained before the subsequent calculations are completed), then $x+X'$, $y+Y'$, $z+Z'$ are the components of a vector, which differs only from the astrographic subject of observation in that parallax has still to be allowed for: moreover the parallax to be used is $\frac{1}{\Delta}$, when

$$\Delta^2 = (x+X')^2+(y+Y')^2+(z+Z')^2.$$

Even in transit-circle observations $\frac{1}{\Delta}$ is the correct parallax, and when, as in the case of Eros, there is a small but sensible difference between Δ and what is obtained by suppressing the accents on X′, Y′, Z′, there is a further obvious convenience in the proposed "Astrographic Ephemeris." P. H. Cowell.

Changes in the Stellar Heavens.

[Continued from p. 402.]

Piscium.—Al-Sufi rated ψ^1 and ψ^2 Piscium of the 4th magnitude, but at present ψ^2 is not much brighter than the 6th magnitude. Ptolemy and Al-Sufi agree in making β Piscium a little brighter than γ, but at present γ is considerably brighter than β. Ptolemy and Al-Sufi make γ and b (Fl. 7) Piscium equal, but γ is now more than a magnitude brighter than b. As these three stars (β, γ, and b) lie near each other and are easily compared, it is evident that some change has taken place in their relative brightness since Al-Sufi's time.

Cetus.—Al-Sufi says nothing of o (Mira) Ceti, the famous variable star, so it was probably faint when he observed the stars of Cetus. Ptolemy and Al-Sufi agree in making α and γ Ceti equal (3rd magnitude), but at present α is about a magnitude brighter than γ.

Eridanus.—The star β Eridani has probably increased in brightness since Al-Sufi's time, as it was rated 4th magnitude by

Ptolemy and Al-Sufi, or equal to λ Eridani, but at present it is slightly brighter than an average star of the 3rd magnitude. Schjellerup identifies Al-Sufi's 17th star of Eridanus with Lalande 4969, which agrees well with Al-Sufi's description of its position. Al-Sufi's estimate of its magnitude—about 6—also agrees well with modern estimates, but the star was rated 4th magnitude by Ptolemy. In the *Philosophical Transactions of the R. S.* for 1786, Pigott notes, with reference to this star, "Fl says he could not see this star in 1691 and 1692. In 1782, 1783, and 1784 I observed one of the 7th magnitude in that place, the relative brightness of which appeared always the same, viz., less than two little stars near and below η Eridani." Both Ptolemy and Al-Sufi make τ^1, τ^4, τ^5, and τ^9 equal (4th magnitude), but at present τ^4 is about a magnitude brighter than any of the others. τ^4 has been suspected of variation. It has a spectrum of the third type.

"The Story of θ Eridani" has been well told by Dr. Anderson in *Knowledge* for July 1893. Al-Sufi gives a very clear description of the position of θ, which he rated of the 1st magnitude. It is now about the 3rd, and there can, therefore, be no possible doubt that this star has diminished in brightness since Al-Sufi's time. The bright star now known as Achernar (α Eridani) is too far south to have been seen by Al-Sufi, and it was—owing to the precession of the equinoxes—still further south in his time.

Canis Major.—The star β, which Ptolemy and Al-Sufi rated 3rd magnitude, is now about the 2nd. The stars δ and ε, which were also rated 3rd magnitude by Ptolemy and Al-Sufi, have also probably brightened, as δ was measured 1·85 at Harvard, and ε 1·49. The difference in the case of ε especially is very remarkable. Al-Sufi rated ζ also of the 3rd magnitude, and this is about its present brightness.

β *Canis Minoris,* which Ptolemy and Al-Sufi called 4th magnitude, is now about the 3rd; but Sir W. Herschel also estimated it 4th mag. on January 9, 1796*. It was rated 3rd mag. by Flamsteed, Argelander, and Heis.

Columba.—Al-Sufi rated θ Columbæ 4th magnitude, and κ 5th mag., but κ is now distinctly brighter than θ, which has probably faded since Al-Sufi's time, as it is now below the 5th magnitude.

Argo Navis.—It is difficult to identify some of Al-Sufi's stars in this constellation, and one or more seem to have disappeared, or at least diminished greatly in brightness. With reference to the stars δ, κ, ι, N, φ, and ε Argûs, these lie so far south—as seen from Al-Sufi's station—that Al-Sufi could only have seen them near the horizon, and then only for a short time when on the meridian. Still his description is very clear. He says that χ, δ, κ, and φ form a straight line, which is quite correct.

Hydra.—Al-Sufi does not mention the stars κ and ψ of this constellation, although they are—now at least—of the 5th magnitude. Both Ptolemy and Al-Sufi make μ Hydræ brighter than λ, but λ is now distinctly the brighter of the two.

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* *Phil. Trans. R. S.* 1796.

Centaurus.—Schjellerup does not identify Al-Sufi's 21st star of this constellation, but from Al-Sufi's description of this and the 22nd star (ξ) there can be no doubt, I think, that the object referred to by Al-Sufi is the well-known globular cluster ω Centauri. Al-Sufi's words are:—"La 21^{e} se trouve devant la brillant 18^{e} [ζ Centauri] et est de la cinquième grandeur; elle est située où commence la croupe du cheval et finit le dos humain. Il y a entre elle et la 18^{e} une distance de deux coudées et demie." This describes exactly the position of ω, as a glance at a star map will show. With reference to the 30th star, which was rated 3rd magnitude by Ptolemy, Al-Sufi says he could not find any star in the place indicated by Ptolemy—about 2 degrees following ϵ Centauri. Al-Sufi says, "Quant à la 30^{e}, Ptolémée rapporte qu'elle suit la 29 [ϵ], qu'elle est de la troisième grandeur et qu'elle est aussi située au dessous du ventre; conformément à son lieu rapporté dans l'*al-madjisti*, il faut qu'il y ait entre elles moins d'une coudée de distance. Mais il n'y a ni en cet endroit ni dans ses alentours d'étoile qui puisse occuper sa place, hormis les étoiles susdites et connues." Here we have a star which probably disappeared, or faded considerably, between Ptolemy's time and that of Sufi. Near the place indicated by Ptolemy there are some small stars. Ptolemy and Al-Sufi agree in making θ and ι Centauri equal (3rd magnitude), but at present θ is considerably brighter than ι.

Corona Australis.—Ptolemy and Al-Sufi agree in making β and ζ equal, but β is now considerably the brighter of the two. Near θ Coronæ Australis Al-Sufi observed a star which seems to have diminished in brightness since his time. He says, "Au devant et près de la 1re [θ], il y a une étoile de la cinquième grandeur, entre elle et la 1re étant, à vue d'œil, une distance de moins d'un empan. Ptolémée n'a pas parlé de cette étoile, quoique elle soit plus brillante que les 11^{e}, 12^{e}, et 13^{e}." Near the place indicated by Al-Sufi there is at present a small star which was estimated 6·3 mag. at Cordoba, Al-Sufi's 12th star (κ) being rated 5·4, and his 13th star (λ) also 5·4. The star near θ has therefore probably faded. It is No. 7731 of Lacaille's Catalogue.

Piscis Australis.—The star ζ of this constellation has probably diminished in brightness in modern times. It was rated 4th magnitude by Ptolemy, 5$\frac{1}{3}$ by Al-Sufi, Argelander, and Heis, but was estimated only 6·7 at Cordoba, and was measured 6·62 at Harvard. Al-Sufi could not have seen the star had it been so faint in his time as it is at present. His description of its position is very clear, and the identity of his star with ζ is beyond all doubt. Schönfeld, in his Second Catalogue of Variable Stars, says, with reference to this star, quoting Schmidt), "U.N. 5–6 m. 1864 Aug. und 1869 unter den günstigsten Umstanden nur mit

Mühe zu sehen, und selbst in dem hochgelegenen Kephissia nur 7 m. in Opernglas." I found it about 7th magnitude in August 1877 and November 1882. From a discussion of the various magnitudes assigned by different observers, C. H. F. Peters came to the conclusion that this star is probably variable with a long period*. It is 6½ mag. in Lalande's Catalogue. In Bayer's map it is shown as 4 mag. (!) Piazzi called it 7 mag., and Taylor 8.

Ptolemy and Al-Sufi agree in making γ Piscis Australis 4th magnitude, and μ 5th magnitude, but μ is now almost exactly equal to γ.

β Gruis was noted 3rd mag. by Al-Sufi, but it is now nearly 2nd mag., and but little less than α Gruis, which Al-Sufi rated 2nd mag. As they are not far apart, Al-Sufi could hardly have made a mistake in their relative brightness.

β and θ Aræ were both rated 4th mag. by Ptolemy and Al-Sufi; but β is now about a magnitude brighter than θ, and slightly superior to α Aræ, which Al-Sufi distinctly states was the brightest star in the constellation in his time.

To show the generally great accuracy of Al-Sufi's estimates of star-magnitudes, I give the following examples (among many others) of his estimates compared with modern estimates and photometric measures:—

Star.	Al-Sufi.	Argelander.	Heis.	Photometric Measures.	
				Harvard.	Potsdam.
ρ Ursæ Majoris	5	5	5	5·04	
h Draconis	5	5	5	4·98	
η Boötis	3	3	3	2·89	3·08
α Equulei	4	4	4	4·11	4·14
η Pegasi	3	3	3	3·06	
μ Andromedæ	4	4	4	3·92	4·09
α Tauri	1	1	1	1·00	
ι Geminorum	4	4	4	4·03	4·03
β Aurigæ	2	2	2	2·07	
ω Piscium	4	4	4	4·16	
ε Ophiuchi	3–4	3–4	3–4	3·37	
ω Tauri	6	6–5	6	5·82	5·76
ζ Tauri	4–3	4–3	4–3	3·77	4·00
θ Canis Majoris	4–5	4–5	4–5	4·23	

The supposed secular variation in the light of certain stars, that is, the slow increase or decrease in their brightness in the course of ages, is a question of considerable interest. In the following tables I give the most remarkable cases I have met with in Al-Sufi's work. Some of these have been already referred to.

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* *Astronomische Nachrichten*, No. 2360.

Stars which have probably increased in brightness since Al-Sufi's time.

Star.	Al-Sufi.	Arge-lander.	Heis.	Photometric Measures.	
				Harvard.	Potsdam
ω Draconis	6	5	5	4·86	
h Herculis	6	5–6	5–6	5·00	4·93
47 Cygni	6	5–6	5–6	4·77	
ρ Cassiopeiæ	6	5	5–4	4·56	
α Ophiuchi	3	2	2	2·18	
σ Serpentis	6	5	5	4·76	
σ Ophiuchi	6	5	4–5	4·42	
δ Sagittæ	5	4	4	3·72	3·94
9 Vulpeculæ	6	5	5–6	4·98	
ζ Delphini	6	5–4	5–4	4·71	4·76
68 Tauri	6–5	5	5	4·24	
γ Geminorum	3	3	2–3	2·00	
ρ Capricorni	6	5	5–6	4·98	
ψ Orionis	6	5	5	4·73	
β Eridani	4	3	3	2·87	
β Canis Majoris	3	3–2	3–2	2·01	
δ " "	3	2	2	1·85	
ε " "	3	2–1	2–1	1·49	
P Argûs	6	......	......	4·11	
δ "	3	......	......	2·00	

Stars which have probably diminished in brightness since Al-Sufi's time.

Star.	Al-Sufi.	Arge-lander.	Heis.	Photometric Magnitudes.	
				Harvard.	Potsdam.
δ Delphini	3–4	4	4	4·57	4·56
τ Andromedæ	4	5	5–6	4·94	5·27
γ Trianguli	3–4	4–5	4–5	4·15	4·32
38 Arietis	4	5	5	5·20	5·44
s Tauri	4	5	5	5·10	5·51
η Cancri	4–5	6	6–5	5·53	5·48
π "	4–5	6	6	5·57	5·53
β Leonis	1	2	2	2·23	2·62
η Virginis	3	3–4	3–4	4·05	
β Sagittarii	2	4–5	......	4·10	
h Aquarii	4–5	6–5	6–5	5·45	
ψ¹ Piscium	4	5–4	5–4	5·03	
ψ² "	4	6–5	6–5	5·72	
27 "	4	5–6	5–6	5·03	
θ Eridani	1	......	......	3·13	
ζ Piscis Australis	5–6	5–6	5–6	6·62	
θ Columbæ	4	......	......	5·36	
β Hydræ	3	4	4	4·18	{4·47 S.M.P.

With reference to stars omitted by Al-Sufi, he has not, I think, omitted any star brighter than the 4th magnitude in the portion of the sky visible from his station. There are, however, a number of stars between the 4th and 6th magnitudes which he has not mentioned. Of these the brightest seem to be π Cephei, ζ Coronæ Borealis, μ and ρ Cygni, π Aurigæ, ε Aquilæ, φ Leonis; ϕ^1 and ϕ^2 Orionis, and λ Canis Majoris. J. E. Gore.

One Hundred Years ago.

On the eve of the commencement of another century, we naturally turn our thoughts to the state of our science at that of the one just concluding, and the additions made to our knowledge in the interim. Here, of course, we can only allude to a few points. When the nineteenth century opened no planet was known to exist between the orbits of Mars and Jupiter; now we are in the fifth hundred of the minute bodies which revolve in that interval of space, besides knowing the existence of a similar body nearer at mean distance than Mars, and when in perihelion very much nearer. Twenty-seven millions of miles was then thought to be the smallest distance to which any planet could approach our Earth; the body in question is now known sometimes to approach us within about twelve millions. Uranus was supposed to be the outermost planet of the solar system; before the century was quite half over a larger planet was known to revolve round our Sun in a nearly circular orbit at 1·58 times the mean distance of Herschel's planet—the first whose discovery is recorded. Mars, hitherto supposed to be moonless, was found to be accompanied by two tiny satellites, one of which revolves round the planet in a shorter time than that occupied by the planet in rotating on its axis. Jupiter, also, besides his four comparatively large satellites (the discovery of which was almost the firstfruits of the invention of the telescope), was found to be possessed of a very small one, much nearer him. Saturn's retinue was increased by one, two years after the discovery of Neptune and nearly two hundred after that of its own largest satellite. Uranus, however, had his supposed number shortened; they are very difficult objects to see, and it would seem that four of the six announced at different times by Herschel had no real existence, but two others were subsequently found, making the whole number known four. In the same year in which Neptune was discovered, one satellite was found to be revolving round him, and no others are yet known.

At the beginning of this century only one comet, that of Halley, had been positively known to return periodically; now two others have returned with periods nearly equal to his, and a considerable number with very much shorter periods. The establishment of meteor-streams in regular elliptic orbits similar to those of comets is entirely an achievement of the present century.

The same may be said of the distances of the fixed stars. All previous efforts to measure any of these had been complete failures; but we now know approximately the distances of five or six, besides having acquired the knowledge of a very large number of cases of orbital motion of stars with respect to each other, which were quite undreamt of till Herschel, in endeavouring to detect stellar distances by comparison of the places of double stars *inter se*, was led to notice how often such were not merely optically double, but were physically connected with each other.

But, in addition to all this and much more, the department now emphatically called the new astronomy is entirely the creation of the present century, and of the second half of it. For it was not till 1802 that William Wollaston first noticed a few of the dark lines in the solar spectrum which have since led, when more fully investigated, to such mighty results, especially as bearing upon the chemical constitution of the heavenly bodies. Astronomy has, in fact, now become a branch of general physics, and the extension of our knowledge of the laws of light and heat has reacted upon all our speculations and theories with regard to the great globes in space both in and beyond our own system.

It is more especially to one point in these that I wish now, with your permission, to allude. All the papers contributed by Sir W. Herschel to the 'Philosophical Transactions' for 1801 are on the solar phenomena. He proposes a complete change in their nomenclature and the adoption of the terms *openings*, *shallows*, *ridges*, *nodules*, *corrugations*, *indentations*, and *pores*. His idea was that the natural state of the Sun, so to speak, was to be spotted; and that "we ought rather to look upon the absence of spots as a sign of deficiency than on their presence as one of abundance." Then he tries to show that corn-crops were usually deficient in seasons nearly free from solar spots, basing his remarks on the tables of the prices of wheat in different years given in Adam Smith's book on the 'Wealth of Nations.' These all relate to the market quotations at Windsor, and Herschel hardly seems to have thought sufficiently of the weakness (not to say futility) of founding an argument on the state of things at one place, unless, indeed, from a much longer course of variations than were available. His proposed new nomenclature was based on an idea that some of the spots were "openings, where, by the accidental removal of the luminous clouds of the Sun, its own solid body may be seen." In fact his views emanated to a great extent from a desire to look upon the Sun as a magnificent habitable globe. I do not think he anywhere alludes to the so-called Wilson phenomenon by name (which Mr. Howlett has done so much in our time to invalidate), but he would seem to have accepted the idea, regarding most of the spots as places where, when we can look straight into them, we see the true dark body of the Sun at a considerable depth below the luminous atmosphere supposed usually to cover it. Our present knowledge of physics leads us, as Miss Clerke expresses it, "to smile at conclusions" which we cannot but regard as "extravagant and impossible," and my chief object in alluding to it now is to show how much our study of other sciences during the century just closing has altered many of our astronomical theories. Probably Herschel was influenced on this subject by his benevolent desire that the solar inhabitants should have the means of studying astronomy; and as they could only see the heavens through the so-called openings, he was led to look upon a much-spotted state of the Sun's photosphere as its usual and, so to speak, natural or normal state. A similar feeling led him, in one of his earliest papers * (in which he shows that the mountains in the Moon were not nearly so high as had been supposed), to speak of "the great probability, not to say absolute certainty, of her being inhabited." I am afraid we can no longer accept the existence of inhabitants, either on the Sun or Moon, however differently constituted to ourselves. W. T. LYNN.

Blackheath, 1900, Nov. 5.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Changes in the Stellar Heavens.

GENTLEMEN,—

Pray allow me a remark on the very interesting article published in the last number of the *Observatory* ("Changes in the Stellar Heavens," p. 401). If the star 61 Virginis was for Ptolemy a naked-eye double-star, and not to-day, it is on account of the very great proper motion of 61 Virginis. This star was near 63 Virginis in Ptolemy's time, and has since been constantly going away from 63 south-westward. So that there has been no change in stellar magnitude.

I am, Gentlemen,
Yours faithfully,
M. MOTE,
Professeur à la Faculté de Droit.

Montpellier, France,
1900, Nov. 14.

[This very interesting note tends to prove the great accuracy of Al-Sufi's observations. When writing my article I was not aware of the large proper motion of 61 Virginis.—J. E. GORE.]

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* Read at the Meeting on 1780, May 11, at which his paper on the variable star in Collo Ceti was also read.

Memorial to the late Mr. G. J. Symons, F.R.S.

Gentlemen,—

On May 31st a meeting was held at the rooms of the Royal Meteorological Society to consider the question of a memorial to the late Mr. G. J. Symons, F.R.S., the distinguished meteorologist and founder of the British Rainfall Organization.

It was resolved unanimously that the memorial should take the form of a Gold Medal to be awarded from time to time by the Council of the Royal Meteorological Society for distinguished work in connection with Meteorological Science.

An Executive Committee was appointed to take the necessary steps to raise a Fund for that purpose. We have now much pleasure in stating that their appeal has been very heartily responded to, not only by meteorologists, engineers, and representatives of other branches of science and industry, but also by personal friends and admirers of the late Mr. Symons in all classes.

The Committee has decided to keep the list open until the end of January 1901, in order to allow all who have in any way benefited by Mr. Symons's advice and assistance to contribute to the Memorial Fund.

Yours faithfully,
C. Theodore Williams, M.D., *Treasurer.*
R. Meldola, F.R.S., } *Secretaries.*
Wm. Marriott, }

70 Victoria St.,
Westminster, S.W.

OBSERVATORIES.

[Continued from p. 413.]

Stockholm. *K. Bohlin.*—Completion of Catalogue of 1211 B.A.C. Stars, and continuation of 2219 Radcliffe Stars. The computations are in a forward state, both for old and new series. Work on small planets. Time service.

Strasburg. *E. Becker.*—The large refractor was used by Dr. Kobold for positions of Comets Wolf (1898 IV.) on 3 days, Chase (1898 VIII.) on 9 days, Swift on 28 days, Tuttle on 2 days, Tempel II. on 11 days, and Giacobini on 12 days. The meridian work consisted of about 300 observations of fundamental stars, and a similar number of circumpolar stars; also a large number of observations of the large planets.

Torino. *Francesco Porro.*—Work on the re-observations of stars in the 'Storia Celeste di Palermo,' in connection with Prof. Hermann Davis. Meteorology.

Utrecht. *A. Nyland.*—Physical observations and drawings of Jupiter. Lyrids and Leonids. *o* Ceti. Time service.

VIENNA (*M. Edler v. Kuffner*). *L. de Ball.*—Light-curves of variables by photography and eye-estimation. Leonids.

ZURICH. *A. Wolfer.*—Sun-spots:—

	Rel. No.	Observation days.	Spot-free days.
1898	26·2	273	30
1899	12·0	292	93

Time service. Leonids.

PUBLICATIONS.

DUNSINK OBSERVATIONS.—Prof. C. J. Joly, Royal Astronomer for Ireland, has recently published Part ix. of Astronomical Observations made at Dunsink, which consists of a catalogue of 321 stars, formed from 1529 observations in Right Ascension and 1512 in Declination made in the years 1898 and 1899 with the meridian circle of the Observatory. Two hundred and five of the stars are stars of reference for objects in Dr. Dreyer's 'New General Catalogue of Nebulæ'; the remainder are heliometer comparison stars, observed at the request of Dr. Gill. The places in both elements are of a differential character, and depend on the star-places in the 'Berliner Jahrbuch.' Proper motions, taken in most cases from Auwers' new reduction of Bradley's observations, have been applied to form the catalogue-place. The volume contains a Ledger, giving the individual places of the stars as found on different nights (which show excellent agreement—a range of $0^{s}\cdot2$ in R.A. or $2''$ in Dec. being rare) and a Catalogue. The observations were made and reduced entirely by Mr. Charles Martin, Assistant in the observatory, of whose care Prof. Joly speaks in terms of commendation.

NOTES.

MINOR PLANET NOTES.—The following planets have received permanent numbers (*Ast. Nach.* 3672):—

Planet.	Discoverer.	Date.	Number.
FD	Keeler	1899 Dec. 6	452
FA	Charlois	1900 Feb. 22	453
FC	Schwassmann	1900 Mar. 28	454
FG	Wolf-Schwassmann	1900 May 22	455
FH	Wolf-Schwassmann	1900 June 4	456

FB proved to be identical with (117) Lomia. FE and FF, discovered by Hirayama in Japan, do not receive permanent numbers, not having been sufficiently observed.

The following planets have been named:—

Planet.	Year of discovery.	Name.
353	1893	Ruperto-Carola.
371	1893	Bohemia.
386	1894	Siegena.

Ephemeris of (433) Eros, for Berlin midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Dec. 4....	1 26 35	49 23	Dec. 20....	1 38 59	43 4
6....	1 26 39	48 40	22....	1 42 19	42 13
8....	1 27 9	47 55	24....	1 46 1	41 21
10....	1 28 5	47 9	26....	1 50 1	40 30
12....	1 29 28	46 22	28....	1 54 21	39 38
14....	1 31 15	45 33	30....	1 58 58	38 47
16....	1 33 26	44 44	Jan. 1....	2 3 53	37 55
18....	1 36 1	43 54			

A. C. D. C.

IN the *Astronomical Journal*, No. 485, Prof. Hussey has another hundred new doubles discovered with the 12-inch and 36-inch refractors at Lick. They are of the same class as his previous list and bear out Prof. Burnham's dictum that the field of discovery is as fruitful as when he began in 1878. Twenty-five of the new pairs are under 0″·5.

THE LEONID METEORS.—From computation of the perturbing effect of the major planets on points in the meteor-stream, Dr. Stoney and Dr. Downing arrived at the conclusion that the orbit of the Leonids does not now intersect that of the Earth, but that the meteors this year pierced the plane of the ecliptic in a point which is about 1,600,000 miles nearer to the Sun than the Earth, and that the only chance of our seeing the meteors therefore lay in the fact that the stream might be wide enough to enclose the Earth. This prediction appears to have been borne out by facts, for there was nothing that could be called a display. At Greenwich watch was kept on the night November 13–14, and in 5 hours 25 meteors were counted, only 5 of which were Leonids; on November 14–15 clouds prevailed, and rain came on about 4 in the morning, but in a watch of 2 hours duration 20 meteors were seen, 6 of which were Leonids. The third night, November 15–16, was the most prolific, and in the watch of 4½ hours 55 meteors were seen, 23 being Leonids, making 34 in all. At Oxford there was rain and bad weather, so that only 3 Leonids were seen altogether. Mr. Denning, at Bristol, also experienced very unfavourable weather; but he says that on November 13 and November 14, observing between breaks in the clouds, the meteors were very rare. Mr. E. C. Willis, writing to

Nature, from Norwich, states that he kept watch, at intervals, from the morning of November 10 until the evening of November 16, and in the total time of 12¼ hours, a considerable portion of which was more or less cloudy, he saw only 42 Leonids, but of other meteors 110. He deduces, from his own observations, that the maximum Leonid shower occurred on the morning of November 14, ending about 17^h 30^m. The hourly rate was then about 13, he says, and would probably have been larger if the conditions had been more favourable. He remarks the extraordinary scarcity of the Leonids on the night of November 15, when only 2 were seen.

With reference to the effect of planetary attraction on the meteor-stream, it may be of interest to recall Le Verrier's words on the subject written in 1867:—"On puit se demander si un changement dans la distance périhélie ne pourrait pas faire disparaître tout-à-fait le phénomène. Mais cela ne semble pas, à cause de l'étendue actuelle de l'essaim. En admittant même qu'il vienne à rencontrer de nouveau Uranus, cette planète n'agira que sur une partie de la matière, et ne déviera de nouveau le tout, comme Jupiter a pu le faire, en 1770, pour le comète de Lexell." —*Comptes Rendus*, vol. lxiv. p. 98.

THE DIAMETER OF THE SUN. — *Bulletin Astronomique* for November contains a discussion of the observations of the recent solar eclipse at Lyons by MM. Ch. André and P. Lagrula. Numerous measures were made of the length and position-angle of the lines joining the cusps.

The following are the deduced errors in the difference of the tabular place of Moon and Sun (Moon's place from Hansen, with Newcomb's corrections; Sun's place from Le Verrier):—

$$\Delta\alpha\,(\text{Moon}-\text{Sun}) = +1''\cdot69 + 0''\cdot703\,\Delta\pi'' \pm 0''\cdot34$$
$$\Delta\delta\,(\text{Moon}-\text{Sun}) = +0''\cdot58 + 0''\cdot583\,\Delta\pi'' \pm 0''\cdot67$$

where $\Delta\pi''$ is the error in the assumed parallax.

The semi-diameter of the Sun is deduced as

$$15'\ 59''\cdot50 \pm 0''\cdot23 - 0\cdot932\,\Delta r_0'',$$

where $\Delta r_0''$ is the error in the assumed semi-diameter of the Moon (which is based on mean value 15′ 32″·83, found by MM. Küstner and Battermann).

It will be remembered that Auwers' value for the Sun's semi-diameter, deduced from heliometer measures, is 15′ 59″·63. This value is used for eclipses both in the 'Nautical Almanac' and 'Connaissance des Temps.'

THE WEATHER IN NOVEMBER.—The month was very mild and damp throughout, and was marked by an almost total absence of fog. The highest temperature was 62°·3 on the 1st, the highest recorded in the month of November since 1895. The mean for the month was 46°·4, being 3° above the average. Rain fell on 19 days, amounting to rather more than two inches and differing slightly from the average amount. The scanty display of November Leonids was observed under fairly favourable circumstances.

VISUAL OBSERVATIONS OF CAPELLA.—Prof. W. J. Hussey has been making a careful examination of this star with the Lick 36-inch telescope, to detect, if possible, its binary character as shown by Prof. Campbell's spectroscopic observations. The spectroscopic results indicated that the most favourable dates for observation would be April 15, June 6, and July 28, as at these times the components would be at their maximum separation; but at these periods, and also on August 2 and 5, no evidence of duplicity could be discerned. With all powers of eyepiece the image appeared quite round.

THE INFRA-RED PORTION OF THE SOLAR SPECTRUM.—In 1894 Prof. S. P. Langley, of Washington, made a communication to the Paris Academy of Sciences on his work with the bolometer on the solar spectrum in the infra-red. At that time he had figured about 100 lines in the spectrum below $\lambda = 1\mu$; but since then improvements in the bolometer have allowed him to extend his researches to $\lambda = 5{\cdot}3\,\mu$, and on September 24 last he presented a note to the Academy (*Comptes Rendus*, No. 19) with a diagram giving 600 lines of the infra-red spectrum between $\lambda = 0{\cdot}8\,\mu$ and $\lambda = 5{\cdot}3\,\mu$, 400 of which are recorded for the first time.

Prof. Langley speaks specially of the telluric lines in the infra-red. These observations have been made during five years between 1895 and 1900, in all seasons, and there are evident systematic changes in the spectrum with the changes of season. The variations are small but well marked. Prof. Langley is far from suggesting that these show as yet any definite relation to weather changes, but thinks this kind of observation should not be neglected by meteorologists. M. Janssen, adding some words of commendation, refers to this as a most interesting point, and hints that these variations may be not unconnected with the ozone in the atmosphere.

MR. LYNN requests us to correct the statement on p. 423 that his 'Astronomy for the Young' and 'Penny Chronology' were published by Messrs. Stoneham. The publisher is Mr. Stoneman, of 39 Warwick Lane, Paternoster Row.

DR. H. S. DAVIS, of the U.S. Coast Survey, has been appointed observer at the station established by the International Bureau for determining Variation of Latitude at Gaithersburg, Maryland, U.S.A.

PROF. G. V. SCHIAPARELLI has retired from the Directorship of the Observatory of Milan, and has been succeeded by his Assistant, Prof. Celoria.

OUR contemporary *Knowledge* is issuing a 'Diary and Scientific Handbook' for the coming year. Price 3*s*. net.

SIR WILLIAM HUGGINS, K.C.B., has been elected President of the Royal Society for the ensuing year.

THE next Meeting of the Royal Astronomical Society will be on December 14; of the British Astronomical Association on December 19.

FROM AN OXFORD NOTE-BOOK.

A FEW months ago a correspondent called attention to an alarming note in a newspaper chronicling the "loss of the Planet Mercury." A cutting has now been sent containing a "Message from Mars." Is there a whole line of steamers with such names? If so, astronomers ought to have been warned, so that they might be on their guard.

> Planet Mars, spoken 6th, 49 N., 37 W., not under control; engines stopped. No assistance asked. (The Planet Mars, s., left Philadelphia Oct. 27 for Bristol.)—New York, Nov. 11.

THERE is an article in the *Strand Magazine* for November on the "first Moon-photographs taken with the great Paris Telescope." The writer, M. François Deloncle, was apparently the originator of the whole idea; he "felt that it was not sufficient that the Exhibition of 1900 should be exclusively an artistic triumph; it must also, if possible, mark an epoch in history by bringing science, which bids fair to completely revolutionize the world in the near future, home to the popular mind," and accordingly he asked himself why a telescope should not be made for the Exhibition "which would bring the celestial bodies almost to our doors?" M. Deloncle found some difficulties in the way, which he surmounted one by one, and the telescope is so far built that photographs can be taken with it, though the visual lens is not yet made. It was a proud moment for him when the night arrived for his first (photographic) observation of the Moon; but the account of his experiences reads quaintly to an astronomer:—

> The Moon had risen now, and her silvery glory shone and sparkled in the mirror.
>
> "A right declension," I ordered. The telephone bell rang in reply.
>
> "Slowly—still slower—now to the left—enough—again a right declension—slower—stop now—very, very slowly."
>
> On the square ground-glass plate before our eyes the Moon's image grad
> crept up from one corner until it had overspread the glass completely.
> there we stood in the centre of Paris examining the surface of our
> with all its craters and valleys and bleak desolation! I had won the

We are carried away by the enthusiasm of M. Deloncle, and almost imagine him the first man to observe the Moon from the centre of Paris. And he has presented us with a new astronomical term. "'A right declension,' I ordered. The telephone bell rang in reply." We are reminded at once of a military command to "right wheel"; of some steamboat direction, followed by the ring of the bell; of a request for some new strange drink at an American bar; and withal, though vaguely, of something astronomical. The phrase is a good one, and perhaps we may be able to find a use for it.

THEY are playing a play in London just now called 'Herod.' One hears of such things even in far-off Oxford, and I learn from an undergraduate whose opinion I much respect (for he has been paid £5 in solid gold for some verses by a London publisher) that Mr. Stephen Phillips, who wrote the above play, is the "only man living who can write blank verse." This is amiable in the critic who writes "considerable" blank verse himself in his leisure moments. However, at present my concern is with Mr. Stephen Phillips, who has written another play besides 'Herod,' called after 'Paolo and Francesca,' two people who lived in Dante, not quite so long ago as Herod, but before Copernicus, not to mention Newton. In this play occur the following lines; they were sent to me with the remark "Newton was evidently about three centuries too late," which seems to be a gentle hint that the author is guilty of anachronism in some degree. But I must leave the magnitude of his offence to be assessed by others:—

O God, Thou seest us Thy creatures bound
Together by that law which holds the stars
In palpitating cosmic passion bright:
By which the very Sun enthrals the Earth,
And all the waves of the world faint to the Moon,
Even by such attraction we two rush
Together through the everlasting years.
S. PHILLIPS: 'Paolo and Francesca,' pp. 111–112.

From the *Western Mail* of October 13 we learn that the Perth Observatory, West Australia, is built, equipped, at work, and ready to undertake the share in the Astrographic Chart which the Permanent Committee suggested in July. It will be a great thing if the programme is thus again filled. Big co-operative astronomical projects have often been started in the past, but seldom carried through to a successful termination, and now that one has been started which bids fair to go through, all help tending to this desirable end is specially welcome. The article in the *Western Mail* is written from the point of view of a sightseer, and we must not treat it as more than demi-semi-official; but we gather that Mr. W. E. Cooke is anxious to get additional help in order to carry out his share of the work properly. He will certainly want it, starting as he does so much behind the other observatories, and we wish him every success. For the same reason I would urge a speedy and earnest beginning, even though the necessary addition to the staff is not immediately forthcoming.

And in wishing good-luck to the Perth Observatory, I would add the names of two other observatories which are starting almucantars. Of the Durham almucantar I knew before, but not of the one at Case College, Cleveland, Ohio. The transit-circle is a beautiful instrument, but if astronomy is to remain an experimental science we must not trust entirely to the transit-circle for fundamental work, and the Directors of these observatories deserve our best thanks for their enterprise in carrying on the testing of Mr. Chandler's beautiful instrument.

An eminent astronomer sends me the following:—

> One of the most glaring of mistaken ideas came to my notice a few days ago. A local amateur astronomer, who has used a telescope for over 40 years with inverting eyepieces, came to me for the places of Borelly-Brooks's Comet. I gave him a diagram with the position of the comet on several nights, to the *left* of Cassiopeia. He came again a few days later saying he could not find it; and on questioning him I found he had been sweeping to the *right* of Cassiopeia because his telescope inverted! He argued that this was perfectly correct, but after awhile admitted he had made a mistake.

There seems no limit to the confusion which inversion makes possible; and it is worthy of remark that my correspondent, in telling the story, uses the phrase "inverting eyepieces," following common usage, whereas he probably means eyepieces which do *not* invert, but leave the inversion due to the object-glass uncorrected. But this, as above remarked, is the usual custom, though it might with advantage be inverted.

One feels almost shy of mentioning the Leonids again. We sat up to look for them two nights of course, but half-heartedly, and rather as people interested in the supper at 3 A.M. than as expectant meteor-observers. We did not invite spectators this year, and only one of last year's volunteers expressed the wish to come again. The *Oxford Magazine* has printed a new poem to the "Faithless Leonids," and one watcher amused himself by making a sort of double anagram, "The Leonids *slide on* till the *oil ends*." Whether he would have made this in broad daylight I am not quite sure. It seems to be a case of "Here endeth the third lesson in patience under disappointment"; and so we return to Novembers of the domestic type without a central sensation. I fear the popular faith in astronomical prediction has suffered a rude shock, but it cannot be helped. The best attempt to explain the failure of the meteors to fall that I have yet heard is that of a domestic cook, who supposed that "they were not ripe enough yet."

From Hall Caine's 'The Scapegoat,' p. 265:—

> It was a wonderful night. The Moon, which was in its first quarter, was still low in the east, but the stars were thick overhead, making a silvery dome that almost obliterated the blue.

A wonderful night indeed!

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